FACULTY OF ECONOMICS AND BUSINESS

THE POLITICAL GEOGRAPHY OF FISCAL TRANSFERS

EVIDENCE USING DATA ON BELGIUM

Dissertation presented to obtain the degree of Doctor in Economics

by

Geert Jennes

Doctoral Committee

Supervisor:

Prof. Erik Buyst	KU Leuven

Co-supervisor:

Prof. Joep Konings	KU Leuven

Other members:

Dr. Klaas Staal	University of Bonn / University of Karlstad
Prof. David Stadelmann	University of Bayreuth
Prof. Tom Van Puyenbroeck	KU Leuven Campus Brussel

Acknowledgements

(Please let me be a little emotional –but I hope not pompous nor dramatic- at the start.) There are 2nd chances in life, and even 3rd chances. That is my finding after having finalised my –unexpected- PhD project. After having not taken the chance to write a PhD at a younger age, I need to thank my promoters, professors Erik Buyst and Joep Konings, for having "imposed" the writing of a PhD upon me 4 years ago as a condition for being able to write policy papers at Vives, the Centre for Regional Economics at KU Leuven. Soon I started to reverse the conditionality: I felt like I was *allowed* to write a PhD as a mostly harmless way of living my midlife, in exchange for writing policy papers along the way. A side benefit was that I was incentivised and disciplined to finally *thoroughly study something*, i.e. the doctum colloquium under which I took classes as another PhD prerequisite.

My public economics-centred study program made me reflect on my time as a student at the University of Antwerp (the former UFSIA), now more than 25 years ago. The "back to school" shock that my PhD work had treated me with sometimes made me think that I had forgotten everything of that time. And it sometimes made me think that most that I had learned at that time, and for some reason still remembered, was wrong. I am grateful for everything that I have learned at KU Leuven during my PhD years. Not the least for seeing what the world looks like "when thinking about it statistically", which we surprisingly rarely do as I came to realise. (As if it would require "slow thinking / the brain's system 2 to take over", as Kahneman (2011) writes.) And not the least for the humbling awareness that understanding reality is at least as much about *how to go about* trying to understand it, as it is about *what exactly* to understand. I could not have imagined these insights at the start of my PhD project. I not only have the feeling that I know *more* stuff now but also that I know stuff *better*. I look at the –henceforth "enlightened"- world differently now. "Yes I can: yes I can know stuff." The latter is not the least thanks to that statistical workhorse called regression analysis. (I recall the thrill of even feeling *a little* like a detective when I had started using it, with the prospect of being able to distinguish reality from words, truth from anecdote, explanation from description, causation from correlation, and cause from consequence.)

The past now seems a strange and in some ways even silly country, in which most things were not only disconnected but also "exogenous", i.e. *given*. Now I know that many more phenomena than I was ever able to suspect are in fact interconnected and "endogenous". I.e. they are the result of human choice, and hence *explainable* (moreover surprisingly often by stubborn political factors even in this 21st century, as I hope to demonstrate below). Nothing is neutral; things are *not* just the way they are; they are the way they are *for a reason*, or at least *for a cause* (and can therefore –perhaps, someday- be changed). I dare to say that, keeping age constant, my PhD endeavour has made me "better, faster, stronger"[1]. At the same time, my stay in academia has made me meet many people who are far "better, faster, stronger" than I am. My PhD experience has to an important extent been an exercise in modesty. (In particular the process of submitting papers to scientific journals has been sobering. At the age of 40-something, I generally took having a paper rejected personally for at least an entire day.) So: sometimes it is *useful* to get "imposed" 2nd chances. In this way I hope to have made up for part of my time lost, e.g. compared to my co-supervisor Professor Joep Konings, who wisely wrote a PhD shortly after we both had graduated from UFSIA in 1989 (in an even better place than at KU Leuven).

[1] A tongue-in-cheek reference to "The six-million dollar man" (also known as "bionic man"), which the older ones of us may fondly remember as a popular television series in the 1970s.

A 2nd reason why I would like to thank my promoters is for having had the confidence that I would be able to demonstrate the discipline and eagerness to successfully finalise a PhD project at my respectable age. However, with hindsight one could even say that my PhD project has rather *brought about* a kind of eagerness to discover, question and learn about reality that will be hard for me to get rid of. (Geekily, this eagerness has also become reflected in the kind of non-fiction books that I have started to collect and read during my leisure time.) In a way, "the ghost has escaped the bottle" and I am afraid that I will even more frequently than before be "inconvenient" drinking company ("older, angrier and wiser"). I have not only learned a lot about public economics in general, but of course also –if I may say so- about my PhD subject itself, i.e. about the question if the geographic distribution of taxing and spending matches the geographic distribution of political power. Persson and Tabellini (2003) already demonstrated the considerable impact of political power relationships, as enshrined in man-made laws, on public finance outcomes. Belgium, that strange, foreign country in which I still find myself living, is a case in point, as I may demonstrate in this booklet.

A 3rd reason for which I wish to thank my promoters is for having given me the freedom to develop my PhD research project largely in my own way. In fact this freedom is the greatest support a PhD student can get. I have thoroughly enjoyed this freedom, although I must admit that with hindsight, it was probably not the most risk-free strategy to give this freedom to me (nor my most risk-free career move to make eager use of it). This is because my freedom made me already soon steer my research away from the areas that most of my colleagues were working on, i.e. into the direction of that old love of mine, politics. Hence already soon I became an economist imperialistically analysing the world of politics, i.e. I started eagerly studying politicians as a class of self-interested entrepreneurs, i.e. the considerable principal-agent problem that representative democracy confronts us with on a daily basis. As a result, several weekends became hardly distinguishable from week days, and many evenings lost their meaning. Luckily for me, the freedom I have enjoyed has not prevented me to present 6 chapters to my PhD committee in April 2015, of which to my delight 5 were found to be scientifically satisfactory. However, one could well say that my race against the PhD clock has been "non-linear" (as well as my learning curve). Indeed, I virtually wrote 2 of my chapters (3 and 5) from scratch only during the very last year of my PhD project, and I broadly finalised 2 (2 and 4) only right before the last year of my PhD project. Moreover, chapters 2 and 4 were only finalised after a rather chaotic process of no less than 20-something revisions (making me increasingly embarrassed of my 1st versions). So, while some of my chapters are to some extent the result of inspiration, most ones are mainly the result of perspiration. (Some researchers are smart, others have to work hard.)

Next to my promoters, I would also really like to thank a number of other people. Let me start "peeling the onion" from the inside. To serial EDC quiz winners Damiaan Persyn, Stefaan Decramer, Wouter Torfs, Jo Reynaerts, Tim Goesaert, Marieke Vandeweyer and Stijn Deruytter: thank you, bright inner circle of former Vives colleagues, for never having visibly lost the patience to help me translate the statistics course of professor Dirk Czarnitzky into practice (although Wooldridge (2009) also helped). Tim Goesaert and Stefaan Decramer may remember my panic when I was first confronted with STATA code. (I will not even mention my first Vives colleagues' continuing help to make me discover the endless possibilities of MS Excel on the side.) Dear former colleagues, you were a "common pool" that I have drawn from extensively. In spite of a KU Leuven PhD in economics being considered the fruit of lonesome work, I would not have obtained it without your intriguingly non-selfish -but typically academic- love of acquiring and sharing knowledge. The responsibility for all remaining errors is mine. The most special thanks goes to Damiaan Persyn, who co-authored the 1st chapter of this PhD, whose nerves of steel managed to make me understand statistics, and who helped me achieve my 1st ever scientific publication at the age of 47.

The next layer of my "onion of support" consists of my somewhat more recent or more distant Vives colleagues. I wish to thank bright young kids Luca Marcolin, Robrecht Vandendriessche, Liqiu Zhao, Koen Breemersch, Jakob Vanschoonbeek, Lieselot Baert, Sander Ramboer, Geert Goeyvaerts, and Jeroen Van den Bosch, as well as Stijn Vanormelingen, Cathy Lecocq, Ilke Van Beveren, Bas van Aarle, Annelore Van Hecke and Greet Lauvrijs for their company and for their accessible help and inspiration in the fields of political economics and statistics. Vives is small, so I needed you all.

As yet another layer of support, I would consider my colleagues and professors at the KU Leuven's Centre for Economic Studies (CES) and Licos, with a special mention of CES's strong and large Public Economics and Governments & Markets groups. To name only a few people: thank you colleagues Toon Vanheukelom (first "things" first), Gizem Hökelekli, Wouter Van der Wielen, Dries Desmet (brilliant on competition economics), Astrid Sneyers, Andrea Guariso, Simon Miegielsen (brilliant on game theory), Liesbeth Colen, Anneleen Vandeplas, Olivia Riera, Ruxanda Berlinschi, Koen Deconinck (brilliant on almost everything, including jobhunt advice), Marieke Franck, Koen Declercq, Willem Sas and Karen Demeyst, for your great minds and inspiring conversations, with respect to a number of you also in the company of several good drinks. And thank you in particular Professors André Decoster and Stef Proost, for fascinating public economics courses (although readers should not look for formal public economics subject matter in this booklet).

I am grateful to mention as a final layer of support the international research community, which I got to know during the various impressive presentations on public finance, fiscal federalism and political economics that I have been able to attend on the occasion of IIPF, EPCS, ZEW, and APSA conferences. I also fondly remember the great drinking and debating evenings in Dresden, Zürich, Taormina, Lugano, Groningen, Mannheim, Dublin and San Francisco. Amazingly, those conferences made me discover that discussing *a colleague's* work is in fact more daunting than seeing *one's own* work being "thrown into the lions' den" (and sometimes being happily torn to pieces). Even more amazingly, some of the researchers whom I met abroad kept responding unselfishly to my incessant requests for feedback and input when back at KU Leuven. To name a few: Robin Boadway, Ivo Bischoff, Maarten Allers, Björn Kauder, the absolutely brilliant Finns Janne Tukiainen and Panu Poutvaara, Charlotte Fridolfsson, Peter Claeys, and Matz Dahlberg. ("Where to start?" were the memorable words this phlegmatic Swede spoke when discussing my 1st chapter with me when I had invited him for a CES seminar in 2013.) Wouter Vermeulen, Thushyanthan Baskaran, Klaas Staal and David Stadelmann stand out as the most patient of the international research community with respect to my questions. While the former 2 understandably and politely declined my offer to become a member of my PhD committee, the latter 2 to my delight accepted. From then onwards, I have taken it for granted that Klaas Staal and David Stadelmann are almost as fascinated as I am about politicians steering fiscal transfers to their constituencies in a country like Belgium, where politics in general and political heterogeneity in particular cast their long shadows on so many aspects of everyday life.

A special thanks goes to Matz Dahlberg, Koen Algoed/Johan Van Gompel, Thushyanthan Baskaran and Stefaan Decramer for being respective "spiritual fathers" of the 1st chapter (fiscal transfers steered by Federal ministers to their electoral district), the 2nd chapter (interregional fiscal transfers resulting from the public debt), the 3rd chapter (the differing electoral incentives that these fiscal transfers may generate), and the 4th chapter (partisan grants allocation to Brussels municipalities). Each of these people has caused me to experience at least one wonderful "Aha-Erlebnis", which by the way makes me realise the potentially enormous impact of wasted time and sheer luck -2 steps forward, 1 step back- on PhD progress[2]. However, the responsibility for all ensuing and remaining errors is of course

[2] The irony of the importance of coincidence in writing a statistical PhD is not lost on me.

mine. A special thanks also goes to David Stadelmann for inviting me to submit my 2nd chapter for publication in his renowned Kyklos journal. Additional special thanks goes to Klaas Staal for agreeing to write a game theoretic article on Belgian debt (and for excellent jobhunt advice). As part of the wider research community I would also count researchers of other Flemish/Belgian universities, in particular Philippe Van Parijs of UCL who usefully questioned my 1st research design and outcomes, and Tom Van Puyenbroeck of KU Leuven Campus Brussel, who upheld his belief in my difficult 4th chapter, and who also agreed to critically read the rest of my PhD as a member of my committee.

A word of thanks also goes to a number of people because of what I have learned from them and their endeavours while I met them during the several side-projects that pleasantly distracted me from my PhD work: Hansjörg Blöchliger and Jaroslaw Kantorowicz of the OECD, Remi Vermeiren, Peter De Roover, Johan Van den Driessche, Jean-Pierre Rondas, Herman Deweerdt, Eric Ponette, Bart Maddens, Theo Peeters, Guy Clémer, Axel Mathot and Karl Drabbe to name only a few of them. I also wish to thank Professor Wim Moesen, for his willingness to chair the Examining Committee at my public PhD defense. Not by coincidence, he was the first ever to teach me (in an excellent way) public finance almost 30 years ago. I wish to reserve my final thought of gratefulness for Danny Geerts, the man who knows everything about public finances and fiscal federalism in Belgium, the world, and beyond, and who never tired of responding to my questions.

I dedicate the result of my PhD work to my daughters, my parents, and my (preferably beer) drinking fellas from my native Boechout/Ranst area. During the years of (spending far too much time on) writing my PhD, I have learned to value their company in a way that I had not imagined. I also wish to pay tribute here to the people around me who passed away over the course of my PhD years. Just in time, I was able to tell Lode Dircken that I had finally come to appreciate mathematics as a PhD student in economics (although some readers may look in vain for some higher-level mathematics in what follows). Lode Dircken was a legend in his own time and my secondary school mathematics teacher during adolescent years that still make me feel their impact. He generously took the risk to advise me at the age of 17 to consider economics as a field of study instead of Germanic languages ("and the rest is history"). Still more importantly, one of my drinking friends was my favourite cousin Koen Vanlommel. He also died in the course of writing this PhD, unfairly soon. I see his spirit living on in his sons, and my tours on the racing bicycle[3] with them are almost as much fun as my tours with him were. Finally, I also lost my favourite aunt, Yvonne Jennes, during my PhD years.

Perhaps writing a PhD means becoming a little less mortal. My 2 dear and late family members have shown me, each in their own way, that having many friends is probably a more effective strategy.

September 2015

References

Kahneman, D., "Thinking, fast and slow". Penguin Books, 2011.

Persson, T. and G. Tabellini, "The economic effects of constitutions". CES, 2003.

Wooldridge, J., "Introductory econometrics". South-western, 2009.

[3] Which have also proven useful to think over a PhD chapter that the day before I finally thought finalised.

Contents

General introduction

"To the victor belong the spoils."

William L. Marcy, New York Senator, 1828

"The Belgian public debt has automatically arisen and will automatically disappear." (translated from French)

Guy Mathot, Federal Vice Prime Minister and Minister of the Budget in 1980-1981 for the Francophone Belgian party PS

"We have to rid ourselves of the Walloon tendency to introduce federal taxes that fall on the Flemish." (translated from Dutch)

Didier Reynders, Federal Minister of Finance from 1999 until 2011 and Federal Vice Prime Minister since 2004 for the Francophone Belgian party MR, interviewed in the Flemish newspaper De Tijd of 24 January 2014

"After the closure of Ford Genk and the ensuing loss of 10,000 jobs, Jo Vandeurzen and Ingrid Lieten, the 2 ministers in the Flemish government hailing from Limburg, 'call the shots' within the newly established Taskforce Limburg for industrial reconversion. In their own words, they are the spokespersons of the province of Limburg in Brussels." (translated from Dutch)

De Standaard of 20 April 2013

In what follows, we will analyse the political geography of fiscal transfers in Belgium. Although the empirical distributive politics and fiscal federalism literature has expanded considerably in recent years, our research is to the best of our knowledge the 1st empirical analysis of the political economics of the geographic distribution of taxing and spending in Belgium, at least when considering the supra-local level. Our central question is if the geographic distribution of the budget matches the geographic distribution of political power and/or of government popularity, given that taxes are raised centrally and politicians are elected locally. In this respect, the case of Belgium is not only under-researched but also remarkable, because of the stark political, cultural and economic heterogeneity of Belgium in spite of its relatively small size. Moreover, our focus on the supra-local government level seems particularly relevant, as the supra-local level comprises the more autonomous government levels of Belgium –in contrast to the municipal and the provincial levels- and as it comprises the government levels that account for the bulk of taxing and spending in Belgium. While local public expenditure in Belgium amounted to 6.3 and 7.1% of GDP in resp. 1995 and 2013[4], supra-local public expenditure in Belgium amounted to 45.7 and 47.4% of GDP in resp. 1995 and 2013, with moreover considerably

[4] See OECD fiscal federalism network database:
http://www.oecd.org/tax/Federalism/oecdfiscaldecentralisationdatabase.htm#C_3
1995 and 2013 are resp. the first and the most recent year for which the OECD provides data.

more variation in the amount of supra-local public expenditure over this time period than in the amount of local public expenditure.

The 1st chapter analyses if net formula-based fiscal transfers from the Federal government to the citizen received at the level of the Belgian electoral districts are to be explained by the size of political representation "received" per district, i.e. by the number of Federal government ministers who have a particular district as their geographic constituency. It is the 1st research we are aware of that establishes a causal relationship between political representation and *formula-based fiscal transfers from the government to the citizen*. It is also one of the 1st researches we are aware of that establishes a causal relationship between *the executive* –which in many democracies is far more powerful than the legislative- and fiscal transfers.

The 2nd chapter stands out from the other chapters in that it simply develops a methodology for calculating implicit net fiscal transfers between the Belgian regions resulting from the Belgian federal debt. We calculate that over the 1970-2002 period the combination of persistent primary surpluses of the Flemish region and persistent primary deficits of the Walloon region –and to a lesser extent of the Brussels region-, all implicit in the *Federal government* budget of Belgium, increased net fiscal transfers due to the federal debt provided by Flanders to Francophone Belgium to no less than 5.6% of Belgian GDP or almost 100% of the total interest bill on the Belgian federal debt.

The geographically very unevenly spread benefits from Belgian federal budget deficits –and hence the geographically strongly diverging incentives for federal budget deficits- may well explain to a considerable extent the large overall size of deficits and debt in Belgium since the 1970s. In other words, interregional net fiscal transfers resulting from the federal debt may be important not just because of their distributional *consequences*, but even more because they may provide an *explanation* for the large overall size of deficits and debt that have particularly characterised Belgium since the 1970s. The geographically apparently strongly diverging incentives for federal budget deficits in Belgium add a new dimension to traditional political economy motives for debt financing. Persson and Tabellini's (2000) *partisan* dimension *over time* to political economy motives for debt financing takes the shape of a government party with more or less polarised preferences compared to the opposition's, and of governments more or less quickly alternating over time. While such political instability may be viewed as polarisation that exists *between governing and opposition parties*, the additional phenomenon that seems to particularly apply to the Belgian federal level is fragmentation, to be viewed as polarisation *within the governing coalition itself*. In contrast to instability, fragmentation adds a *simultaneous partisan* dimension to political economy motives for debt financing in Belgium. The additional potential consequences of the Belgian *simultaneous geographic* dimension have hardly been investigated in the existing literature on the political economy of budget deficits.

In a 3rd chapter, we take up the challenge to empirically explain the *overall size* of Belgian federal deficits and debt by the *geographically unevenly spread costs and benefits* stemming from deficits and debt. While logically following the 2nd chapter, also this chapter stands out from most of our other chapters. This is because it takes the taxing-and-spending variable as the *independent* variable of interest, rather than as the dependent variable. In this 3rd chapter we investigate more in particular if an increase in net formula-based fiscal transfers from the Federal government to its citizens –the dependent variable of the 1st chapter- increases the Federal government's popularity at the administrative district level. This research stands out from the existing literature on the relationship between fiscal transfers and government popularity in that it uses *(1) net (2) formula-based* fiscal transfers received per district as the independent variable, which is argued to be a measure of a

particular district's *(3) contribution to the federal budget deficit*, and therefore to the federal public debt.

In contrast to the existing literature, in this 3rd chapter we hypothesise that an increase in fiscal transfers will increase government popularity more in the part of a country for which the federal public debt has a relatively low cost -Francophone Belgium in the case of Belgium- than in the part of a country for which the federal public debt has a relatively high cost -Flanders in the case of Belgium. Such a different effect on government popularity could then be an explanation for the size of the public debt in Belgium, which has been large ever since the 1970s[5]. To the best of our knowledge, it is the 1st empirical investigation of the effect of *geographic* fragmentation of a central government on taxing and spending. Previous research on the political economy causes of central government debt mainly investigated if fragmentation in terms of *the number of parties* represented in the government *as such* , as well as if government instability –in terms of the short-lived-ness of a government- would increase central government debt. To the best of our knowledge, it is also the 1st empirical investigation of geographically differing preferences of taxing and spending within one and the same country.

The 4th chapter analyses if discretionary grants allocated by a Belgian subsidising supra-local government to a municipality that is part of the Brussels region are to be explained by the degree of partisan alignment of that municipality with the supra-local grant awarding governments. This research stands out from most of the existing literature on the relationship between partisan alignment and allocation of grants in that it investigates the effect of partisan alignment *per individual party* that is part of a government coalition –either at the subsidising level or at the grants-receiving level. Most of the existing literature -more crudely- only considers the alignment of entire governments –either single-party or coalition governments- without being able to identify the contribution of individual parties. For this reason, this literature is unable to model alignment –in a more refined way as we do- as an interaction variable.

The 5th chapter stands out from the other chapters in that it does not explain the geographic *distribution* of the government budget in Belgium, but its overall *size*. It analyses if an increase in the number of politicians governing at one of Belgium's supra-local governments increases public spending and/or the budget deficit, an effect that is known as the Law of 1/n in the political economics literature. This research stands out from most of the existing literature on the relationship between the number of politicians and the size of government in that it also investigates the effect of the *size of the executive* (from various angles) on public spending, and not only the effect of the size of the legislative (from various angles). The size of the legislative is the traditional independent variable of interest in the –until now rather limited- existing literature on the Law of 1/n.

References

Persson, T. and G. Tabellini, "Political economics". MIT Press, 2000.

[5] Indeed, constitutionally Flanders and Francophone Belgium are guaranteed equal representation in the Federal government, and while cooperation of *all* coalition partners in a particular government may be needed to *avoid* a deficit, non-cooperation by *one* of them may suffice to *cause* a deficit.

Chapter 1: The effect of political representation on the geographic distribution of income: evidence using Belgian data[6]

Abstract

This paper investigates the role of political representation in explaining geographic variation in social security and income tax transfers. To quantify the causal link going from political representation to transfers, we identify 2 sets of exogenous changes in political representation and use an instrumental variable approach. Using Belgian data for the 1995-2010 period, we find that transfers to inhabitants of a particular electoral district increase significantly with every extra federal minister originating from that electoral district. Given that social security and income tax transfers appear to be largely formula-based this result is surprising.

Keywords:

distributive politics; income redistribution; fiscal transfers; political representation

[6] This paper has been jointly written with Damiaan Persyn and has been published as: Jennes, Geert and Damiaan Persyn, "The effect of political representation on the geographic income distribution in Belgium: evidence using Belgian data", European Journal of Political Economy 37 (2015): 178-194. It has greatly benefitted from critical comments and suggestions from Thushyanthan Baskaran, Andreas Bernecker, Erik Buyst, Liesbeth Colen, Matz Dahlberg, Koen Deconinck, Klaus Desmet, Andrea Guariso, Joep Konings, Bart Maddens, Jørn Rattsø, Klaas Staal, Marc Swyngedouw, Philippe Van Parijs, Tom Van Puyenbroeck, participants at the 2013 EPCS Conference in Zürich, at the 2013 IIPF Conference in Taormina, and at the 2013 Licos seminar series in Leuven, 2 anonymous referees of the European Journal of Political Economy, and our colleagues at Vives. We thank the National Bank of Belgium, the Belgian federal Ministries of Pensions and of Employment, Statistics Belgium, Jo Buelens (VUB) and the federal Parliament of Belgium for data provision.

1. Introduction

This paper investigates the role of political representation in explaining geographic variation in social security and income tax transfers. For many countries there exists scientific evidence of political manoeuvring in allocating *discretionary* expenditures geographically (see e.g. Dahlberg and Johansson (2002) for Sweden and Knight (2008) for the US). However, in this paper we look at the geographic distribution of the income tax, social security contributions and social expenditures, which are firmly based on legislation, and therefore –presumably- *formula-based*. The social expenditures we consider comprise all major social transfer categories: pensions, unemployment benefits, child allowances, work disability allowances, allowances for the handicapped, and subsistence allowances. In 2010, income taxes on average amounted to 8.4% of GDP in OECD countries, while social security contributions on average amounted to 10.1% of gross wage earnings, and social expenditures on average amounted to 22.1% of GDP[7]. The fiscal transfers we consider are clearly economically very meaningful in size, but have nevertheless received little attention in previous research, perhaps because they are thought to be mainly formula based and therefore mostly immune to tactical manipulation.

Tax and social security systems may be less formula-based than they appear at first sight, however. Firstly, politicians may cater for their constituency by introducing and changing legislation. One example would be the creation of target group measures in social security (e.g. employment schemes and rebates in social security contributions) and in the tax system (e.g. targeted tax rebates). These types of measures are an important element of social security and taxation systems in most countries. Secondly, even within apparently formula-based systems such as social security, some decisions remain discretionary. An example for Belgium is the fact that the agreement of the federal minister of employment is required to secure exceptional state-funded early retirement for employees of any firm that has gone out of business. Thirdly, the practical implementation of legislation might be influenced. Politicians may prod local branches of the tax and social security administrations towards lenient taxation and social security audits. In Belgium, considerable geographic disparity in the number of tax and social security audits has been found to exist (Deloitte 2010). Lastly, the practical organisation of the executive in most countries involves regular meetings between ministers, offering plenty of scope for negotiation and bargaining such that a minister with competencies unrelated to tax or social matters can nevertheless weigh on decisions related to these topics affecting his/her constituency. It is important to keep in mind that although we believe the above channels may be at work and could at least partially offer an explanation of the empirical regularities we observe, our results below do not constitute proof of any of these channels in particular to be operative.

Motivated by the fact that in most countries the executive dominates the legislative, the focus of our attention will be on political representation within the executive power. This stands in contrast with the bulk of the distributive politics literature where the focus has been on parliamentary representation. To identify causal effects of political representation on transfers we perform an instrumental variable analysis using a Belgian dataset for the years 1995-2010. A first approach we use is to exploit the resignation of ministers following various events deemed orthogonal to transfers. In a 2nd and separate IV analysis, we exploit the unique opportunity offered by the changes in ministerial representation that occurred due to the redrawing of the Belgian electoral district borders in the reform of 2002.

[7] See www.oecd.org

The remainder of this paper is structured as follows. Section 2 gives an overview of the distributive politics literature in the context of which our paper is to be situated. Section 3 describes the data we use in our analysis. Section 4 consists of the actual empirical analysis of social and fiscal transfers and the quantification of the effect of ministerial representation. Section 5 draws a number of conclusions.

2. Related literature

Our analysis differs from the mainstream distributive politics literature because of the combination of our choice of the independent variable of interest (political representation in the executive), the dependent variable we consider (formula based transfers), and our empirical strategy (using instrumental variables to control for reverse causality). In the following paragraphs we discuss these 3 differences in detail.

2.1. Political representation in the executive versus in the legislative

The bulk of the literature on the effect of political representation on fiscal transfers has so far focused on districts' representation within the legislative, rather than within the executive. Within this literature, most research has focused on representation within US Congress. Some of this literature looks at districts' representation in Congress across-the-board, i.e. without distinguishing if a district is represented by a Congress member belonging to the majority or not. An example is the research by Ansolabehere e.a. (2002, 2003) on the effects of representation of districts compared to their population size. Examples for Canada are Evans (2005) and Jutras and Vaillancourt (2008). Dragu and Rodden (2011) perform a cross-country study on the effects of representation of districts compared to their population size in federations over the world.

A number of studies explicitly considers the effect of whether a district voted in favour of the majority in US Congress on transfers received. Examples are Albouy (2013) and Young and Sobel (2013). Guccio and Mazza (2013) take a similar perspective for a region of a European country, i.e. Sicily. Studying different countries, Bickers and Stein (1996), Dahlberg and Johansson (2002), and Helland and Sørensen (2009) focus on the narrowness of the vote margin by which the parliamentary majority "holds" a particular district. The latter studies find that so called "swing districts", i.e. districts represented by vulnerable seats, receive more transfers.

However, in many countries, the legislative is dominated by the executive –and in particular by cabinet ministers-, as well as by political parties and their presidents (see e.g. Timmermans 1994 and Dewachter 2001 for case studies of Belgium). The role of political parties in determining transfers is more closely investigated by a series of studies that analyse the effect of political alignment. More in particular, these studies investigate if more transfers result from similarity between the district's lower level government and the transfer providing higher level government with respect to their party-political composition. Examples of studies of such alignment effects are Brollo and Nannicini (2012) for Brazil and Migueis (2013) for Portugal.

It is surprising that only 3 papers in the distributive politics literature investigate the effect of district representation by one or more individual members of the executive, i.e. by cabinet ministers on

transfers[8]. Crampton (2004) and Milligan and Smart (2005) do so for Canada. Both find that swing districts held by a minister receive more grants, and also –but to a lesser extent- that the governing party's swing districts in general receive more grants. This extra support goes at the expense of the governing party's core districts. Similarly, Golden and Picci (2008) find for Italy that ministers' districts[9] receive more grants at the expense of the governing parties' core districts.

Given the political predominance of the executive in many countries, we will focus on the geographic distribution of government ministers when looking for a political explanation of the geographic distribution of fiscal transfers in our empirical application to the case of Belgium.

2.2. Net fiscal transfers versus discretionary spending

In the overwhelming majority of the existing political economy research on the geographic distribution of grants, including in the majority of the studies cited above, the dependent variable is not formula based expenditures but rather discretionary expenditures. These concern mostly investment grants, i.e. grants for local public goods, sometimes labelled as earmarks or "pork barrel". This is also the case for the 3 abovementioned studies that explain the effect of ministerial representation. This focus on discretionary spending is understandable, as this spending –almost by definition- can be steered towards specific geographic areas, and therefore would be more easy to manipulate by political agents. E.g. Crampton (2004 p. 5) reports that approval of the member of parliament elected for a particular Canadian district –who in some cases was a cabinet minister- was needed for effective disbursement of the expenditure category he studies, i.e. grants for SME development.

An important problem with discretionary spending, however, is that this type of expenditure represents a relatively small share of total redistributive expenditure in most OECD countries. E.g. in Belgium public capital transfers and gross public investment taken together amounted to about 3.8% of GDP in 2010, compared to social expenditures amounting to 29.5% of GDP[10]. We therefore chose to rather focus on explaining social security and income tax transfers, which are likely harder to steer but economically more relevant.

Few empirical studies explain the geographic distribution of these formula-based transfer expenditures or of taxation by political variables. There exist some studies, however, that substantiate the qualitative claim by Besley and Coate (2003 p. 2628) that "even when spending is allocated formulaically, it is possible for legislators to manipulate such formulas to favour their own districts". E.g. Sorribas-Navarro (2011) finds evidence of a causal relationship running from the proportion of swing voters of a Spanish region with high budget deficits towards extra -a priori formula-based- intergovernmental grants. Sorribas-Navarro explains this causal relationship by the fact that the formula determining these grants is open for renegotiation between Spanish regions every 5 years. A 2nd case in point is Litschig (2012), who finds evidence of manipulation of particular municipalities' population estimates underpinning formula-based grants to municipalities by the main incumbent

[8] However, some US studies distinguish between political representation as such and representation by particularly powerful members of a political party. Examples are Knight (2005 and 2008), who investigate the effect of proposal power that some members of Congress have thanks to their membership of Congress committees whose expenditure impact is large.

[9] As well as districts of other powerful party members, such as party presidents.

[10] See www.oecd.org

party in Brazil during the early 90s. A final example is Banful (2011) who finds a similar manipulation of the formula that determines the distribution of intergovernmental grants in Ghana[11].

In contrast to these studies however, our dependent variable concerns grants between government and citizens and not intergovernmental grants. This makes our research more closely related to studies like the one by Atlas e.a. (1995), who find that US states that have a higher Senate representation per capita receive more federal spending per capita, including more so called entitlement spending –i.a. social security spending[12]. That the size and significance of the entitlement coefficient are the lowest of all expenditure categories considered does not surprise Atlas e.a. (1995 p. 628) as entitlements are less easy to control or target to a state.

A 2nd closely related study is Evans (2005)[13], who finds that personal income taxes paid per Canadian electoral district between 1961 and 1999 depend on representation in the House of Commons, in terms of seats share per capita. Evans (2005 p. 279) points at the existence of tax deductions favouring inhabitants in remote and sparsely populated districts, where the number of votes needed to win a seat is lower[14].

2.3. Empirical strategy

A central weakness in most of the literature cited above is that it does not take into account the fact that the political variables of interest might be endogenous, i.a. subject to reverse causality.

A study in distributive politics that does tackle the endogeneity issue is Albouy (2013), who makes use of a Regression Discontinuity Design (RDD) to identify instances in which gaining power at the US state level is approximately random. Also Brollo and Nannicini (2012) make use of such an RDD to study the effect of political alignment on intergovernmental grants in Brazil. Another exception is Carozzi and Repetto (2013), who explain transfers to Italian members of parliament's birth towns that lie outside these MPs' electoral district, and hence cannot be subject to reverse causality.

The 3 studies considering the effect of ministers' origin on the distribution of expenditures that we discussed above (Crampton, 2004; Milligan and Smart, 2005; Golden and Picci, 2008) assume exogeneity of ministerial representation. Of the 3 studies only Milligan and Smart (2005 p. 14) motivate this assumption, by claiming that, because in a strong party system like Canada's ministers are appointed by their prime minister, they are exogenous to transfers[15]. A similar reasoning could be applied to a strong party system like Belgium's, where party presidents appoint ministers. However,

[11] At first sight Levitt and Snyder (1995) and Padovano (2012) also explain formula-based transfers –social security expenditures to US citizens and intergovernmental equalisation transfers to Italian regions resp. - by political economy factors, but their coefficients turn insignificant when these authors add electoral district fixed effects.

[12] Atlas e.a. (1995 p. 627) also find that these states do *not* pay less federal taxes per capita –which the authors explain by the fact that the tax code is infrequently overhauled and largely income-based.

[13] Evans's findings are confirmed by Jutras and Vaillancourt (2008).

[14] Finally, there is the claim by Alesina (1994 p. 31) that invalidity pensions –which may be assumed to be non-discretionary- have been used as "a transfer system from Northern to Southern Italy". In 1984, the ratio of invalidity pensions over old age pensions in the whole of Italy would have been 40%, but it would have been 250% in Southern Italy.

[15] Instead Milligan and Smart (2005) develop an IV estimation strategy for vote share per district, their other major independent variable of interest.

we argue that political representation could nevertheless be endogenous due to reverse causality. It is likely that, even in a strong party system, when a candidate obtains many preferential votes[16] during the preceding election, his party president will find it harder to deny this candidate a ministerial post[17]. Obtaining many votes could in turn be due to having channelled extra transfers to one's district[18], as e.g. demonstrated by Stratmann (2013) with respect to earmarks voted by US Congress. As a matter of fact, it is hard to imagine a mechanism where ministers would channel transfers towards certain districts, if not for attracting votes and either directly or indirectly affecting the probability of re-election or re-appointment.

For reasons of prudence, i.e. to be sure to isolate the causal effect of ministerial representation on transfers toward a district, we therefore will not assume that ministers are appointed exogenously. Instead we will instrument our ministers variable using changes in ministers that may be reasonably thought to be independent from transfers. We develop 2 alternatives to isolate such exogenous minister changes.

3. Data

This study considers cash transfers from and to households through social security contributions and social expenditures and personal income taxation. Social security benefits in kind, i.e. health care expenditures, are not included, due to lack of data. As we aim to make use of the exogenous variation in ministers due to the redrawing of electoral districts in 2002 in Belgium, our analysis will focus on this country. All data on transfers was obtained from the NBB website[19] for the period 1995-2010 at the administrative "arrondissement" level of geographic disaggregation. The type of transfers we consider represent a large share of geographic fiscal transfers, and their budgetary impact is large. At the contribution side it comprises 2 of the 3 by far most important sources of funding of the Federal government, i.e. the personal income tax and social security contributions. Taken together, the transfers considered reduced interregional average household income differences by 33% in the year 2005. (Persyn and Algoed, 2011)

Graphs 1a (for the 10 Flemish electoral districts and the mixed Brussel-Halle-Vilvoorde district) and 1b (for the 9 Francophone electoral districts) show the evolution of cash transfers through social security contributions and expenditures and personal income taxes over time per electoral district,

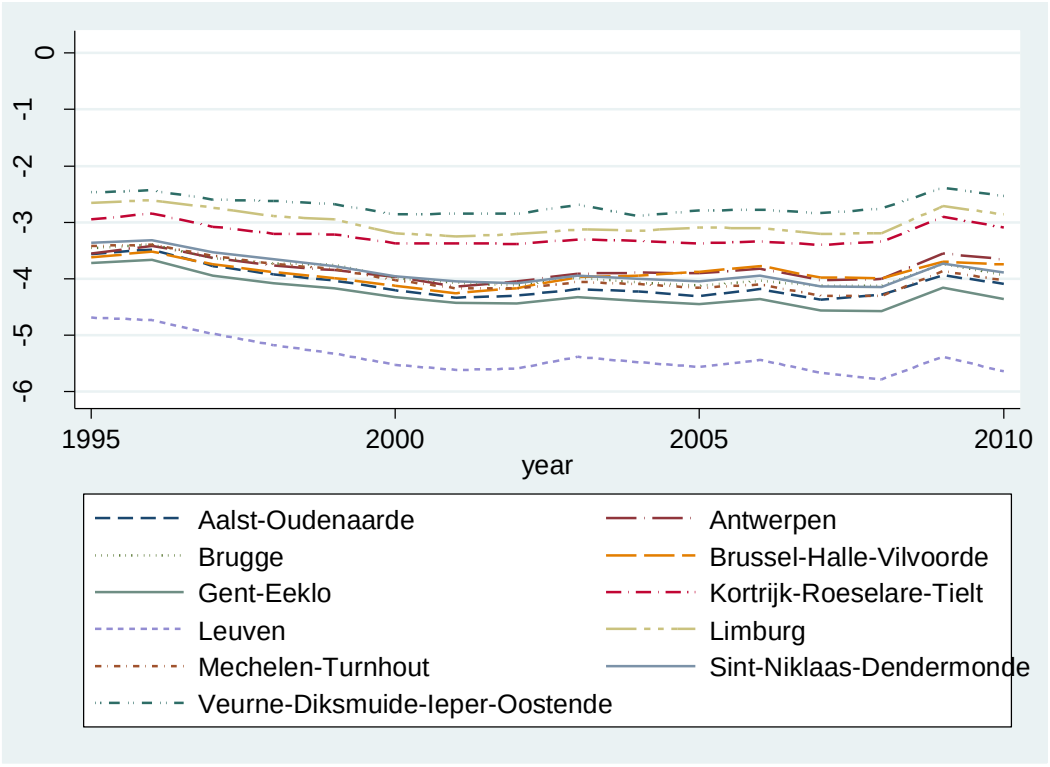
[16] In Belgian elections, voters can either vote for a party in general, or for one or more candidates on the candidate list submitted by a particular party. The latter votes are called preferential votes. The number of preferential votes obtained by key party members such as outgoing ministers are compared to each other in the media, and are reportedly used within parties in the struggle for minister postings. Golden and Picci (2008 p. 270) mention a similar mechanism within DC, the most powerful party in postwar Italy until its collapse in 1993.

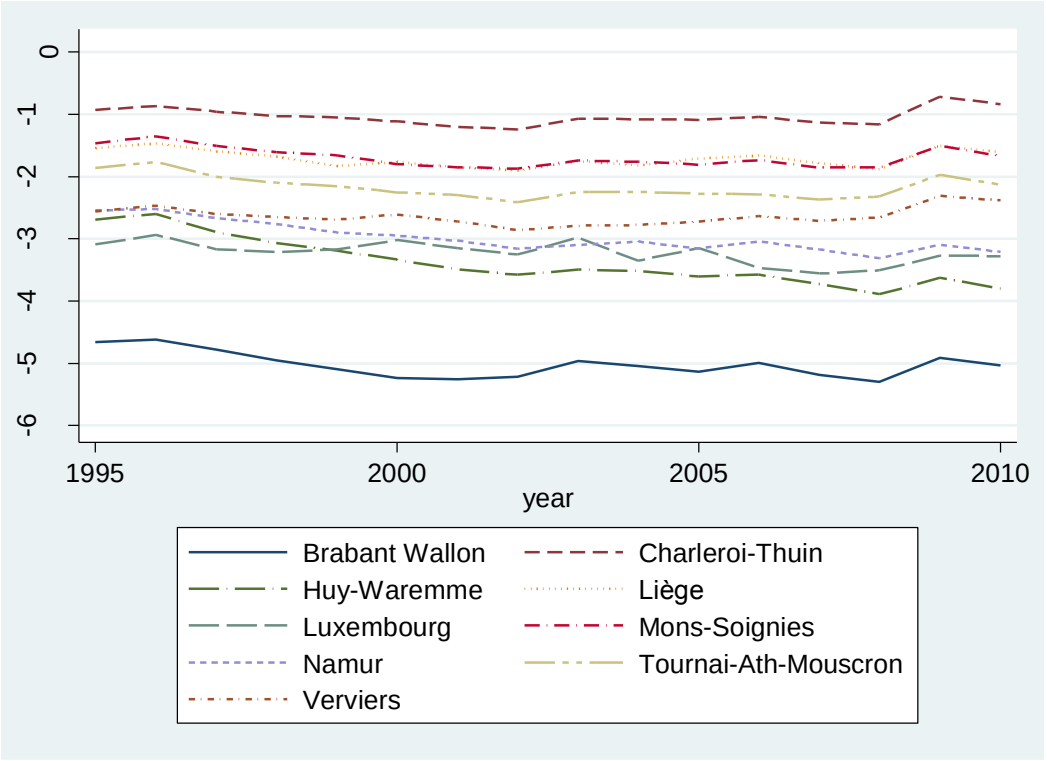
[17] In Belgium little is known about the criteria party presidents use for the awarding of ministerial posts. Reportedly an intra-party balance would have to be struck between the various party factions, but not between electoral districts. (see e.g. Timmermans 1994 p. 110-111) As a result some districts provide more ministers than others.

[18] But it is also true for Belgium that more preferential votes are obtained by candidates ranked high on the list, and that a candidate's ranking is determined by the party (i.e. to a large extent by the party president).

[19] www.nbb.be/doc/dq/n/dq3/NNR.pdf; this is the only source of geographically disaggregated Belgian federal revenue and expenditure data we are aware of.

Graphs 1a and 1b: The evolution of fiscal transfers per electoral district, in real 1000 euro per capita terms, for Flanders and Francophone Belgium resp. (1995-2010)





Source: NBB

expressed in 1000 euros per capita terms, and deflated by the consumer price index for Belgium[20]. It is clear from the graphs that all electoral districts are net payers to the federal state. This is mainly due to the fact that our fiscal transfer data comprise the personal income tax and social security contributions at the revenue side, and only (part of) social benefits at the expenditure side.

The graphs show large differences in net transfers between districts, but perhaps most striking is the overall persistence of the relative positions of the electoral districts over time, with a number of Francophone districts persistently contributing the least of all districts to the Federal government, and a number of Flemish districts persistently contributing among the most of all districts to the Federal government. These large and persistent differences are caused by the equally large and persistent income differences between the typical Francophone Belgian electoral district and the typical Flemish electoral district. The persistence of large between-district-differences in net transfers will pose a challenge to our attempt in the analysis below to explain the relatively small within-district-changes in fiscal and social transfers over time by political factors. Nevertheless, some electoral districts did fare better or worse than others over time, with some changes in relative positions as a consequence.

As for our political variable of interest, graphs 2a and 2b show the evolution of the number of federal ministers per 100,000 inhabitants for the separate pre-2002 electoral districts. Data was obtained from the federal parliament of Belgium. For election years the distribution of ministers of the outgoing government was used. Federal elections were always held in May or June over our sample period[21]. However, in the analysis below, for the years after 2002, when the "provincial" districts reform was voted into law, we assume ministers distribute their attention among the smaller old districts contained in their new provincial district in proportion to the population contained in the smaller districts.[22] Assuming instead that ministers continue only to cater for their old smaller district does not materially affect any of our results.

Graphs 2a and 2b not only reveal considerable variation between electoral districts in terms of the number of ministers elected in a particular district per 100,000 inhabitants, but also –in contrast to the transfers shown in graphs 1a and 1b- considerable variation over time. In 62 of our 320 district-year observations, the absolute number of ministers per district changes. The graphs show that –unsurprisingly- changes in (the provenance of) ministers occur mainly in the years following federal election years (i.e. in years 1996[23], 2000, 2004, and 2008). The red vertical bars in the graphs mark federal election years.

But there also seem to be a lot of exceptions to this rule, i.e. 33 out of 62 minister changes did not occur in post-election years. This is because, as opposed to members of parliament, politicians are not elected to be a minister (nor re-elected) for an entire legislature, but appointed (and dismissed) for a non-predetermined time period. Formally appointments are made by the head of state, but de facto the positions are divided between parties after the elections during negotiations between party presidents.

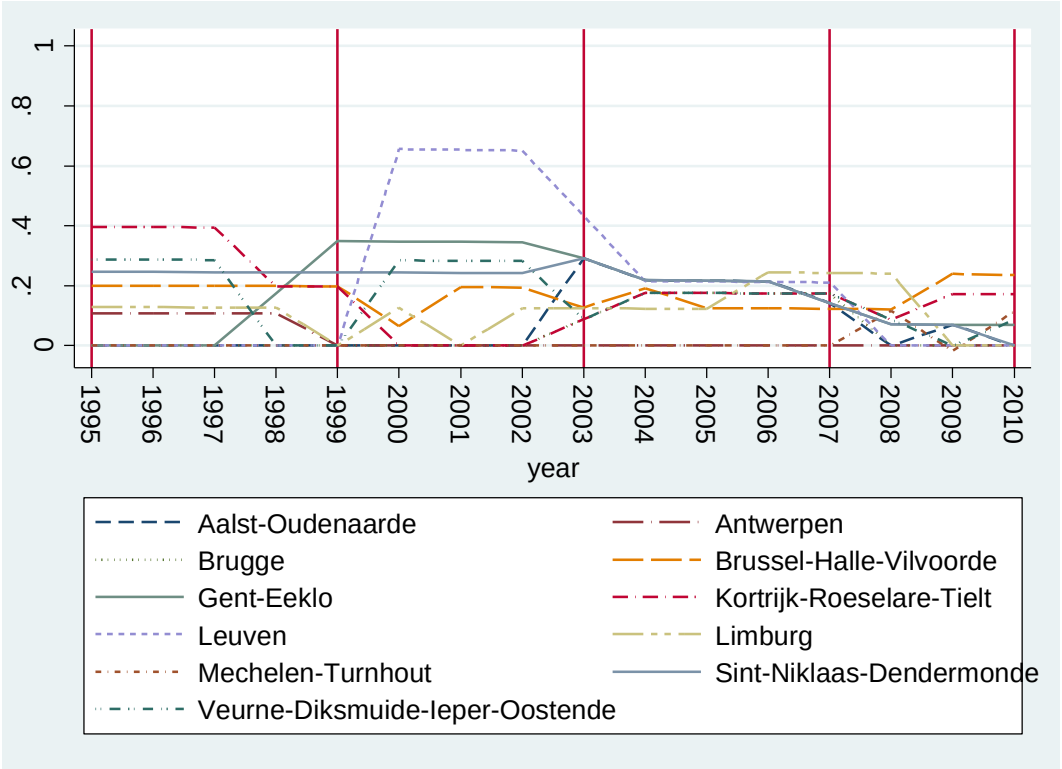
[20] Taken from Statistics Belgium (http://statbel.fgov.be/).

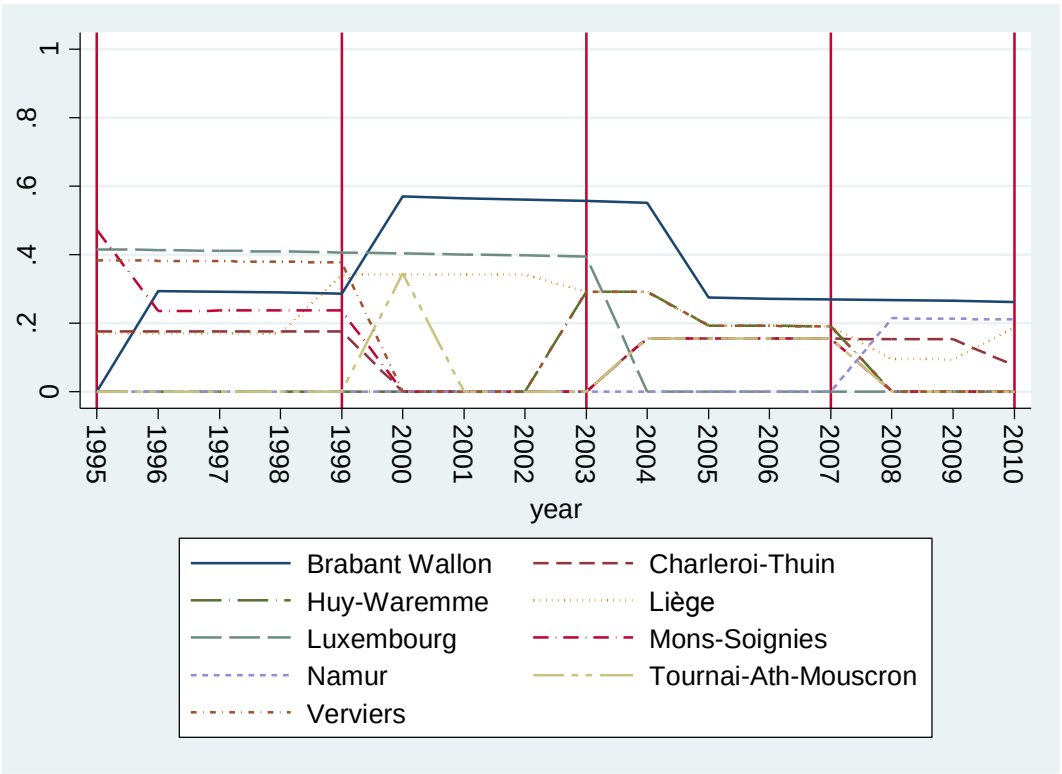
[21] Assignment of ministers in election years to the outgoing government has the advantage of taking into account the possible existence of a political business cycle, which could imply that ministers focus their redistribution efforts on the period preceding an election.

[22] For example, a smaller district with 100,000 inhabitants having one minister originating from that district, and which represents 1/3 of the total population of a newly instituted larger district, has a value of ministers (which is the number of ministers expressed per 100.000 inhabitants) of 1 when ignoring the reform, and a value of ministers of 1/3 after recalculating the variable to take into account the redrawing of constituencies.

[23] As the result of the 1995 elections was the continuation of the outgoing Federal government coalition, minister changes were very limited in 1996.

Graphs 2a and 2b: The evolution of the number of ministers per electoral district, per 100,000 inhabitants (1995-2010)





Sources: Federal parliament of Belgium, own calculations

When deemed necessary, party presidents replace a minister in between elections or a minister himself/herself chooses to resign. The large number of changes in political representation that happen between elections inspired us to consider a subset of them as exogenous to fiscal transfers and use them as instruments to identify a causal effect of political representation on transfers in section 4.

4. Empirical analysis

4.1. Empirical strategy

Our estimation equation is given by

$$transfers_{it} = b_0 transfers_{it-1} + b_1 ministers_{it-1} + b_2 log(grossincome)_{it} + b_3 dependants_{it} + e_{it}.$$

Here, $transfers_{it}$ denotes the fiscal transfers per electoral district i and year t, in real 1000 euro per capita terms. Our specification with a lagged dependent variable (LDV) allows to differentiate between the short and long run association between political representation and transfers, and conforms to the intuition that the amount of transfers towards a district might be slow to adjust and may be conditional on past levels even when taking into account the different covariates in our model. Our independent variable of interest is *ministers*, the number of ministers per 100,000 inhabitants. A single one-year lag of the *ministers* variable is used, as it is likely that ministers require some time in office before they may start to affect decision-making and transfers. We further control for gross income per capita in logarithms, *log(grossincome)*, and the number of *dependants* (minors, retired, unemployed).

We further specify the error term as $e_{it} = z_i + q_t + m_i\ t + p_{it}$. The inclusion of a full set of district dummies z_i removes any bias by omitted time-constant factors (e.g. those related to political and economic history) within electoral districts, which may be correlated with both the amount of transfers and our political variable of interest, i.e. ministers per district. This is equivalent to a "fixed effects within estimation" considering only variation in the data within electoral districts by subtracting the district-specific means for each variable. Moreover, we add year dummies q_t to control for any effects common to all districts in a specific year, and district-specific year trend effects m_i (interacted with the year t) to further control for any omitted trending variable that might be specific to a district[24]. All reported standard errors are grouped (clustered) on the district level, allowing for heteroskedasticity and arbitrary autocorrelation between observations within each district.

Our hypothesis is that ministers cater to their constituency in order to gain votes, and that the electorate in turn rewards ministers for their services. For the case of Belgium, this would work as explained above through preference votes, which in turn are conducive to ministerial (re-)appointment. If such a mechanism of mutual causation is indeed at work, using OLS to estimate the above estimation equation will result in biased estimates, due to the fact that the *ministers* variable then is endogenous and correlated with the error term.

We will first ignore such possible simultaneity bias and estimate equation (1) using OLS. These results do not have a causal interpretation, as they do not reflect the effect of an exogenous change in

[24] The estimation results for these dummies and district-level year trends are omitted from all tables.

the ministerial representation of a district on the incoming transfers. Nevertheless, we find it important to report these results, as finding a significant effect in the single equation setting suggests that some political-economy mechanism may be at work.

4.2. Estimation results when ignoring the endogeneity of political representation

Column (1) in table 1 presents the estimation results when ignoring the endogeneity of political representation and estimating equation (1) using OLS. Column (1) shows cash transfers to an electoral district to be 9 euros higher per capita and year, for every federal minister originating from that electoral district. As expected, a higher average gross income is associated with lower transfers to a district, and a higher share of dependent inhabitants is associated with more transfers. The gross income variable is highly correlated with many key socio-economic variables, making it difficult to separately estimate the relation between other important socio-economic variables and transfers after controlling for gross income. The share of dependants has a correlation coefficient of -0.85 with gross income, and in some specifications that we will discuss below both variables will not be jointly significant. The inclusion of 20 district dummies and 20 district-level year trends also implies that a lot of variation within districts is already accounted for in all specifications, making it hard to additionally estimate the effect of slowly changing variables such as income or the share of dependants (unlike the more erratically changing political representation).

Table 1: Estimating the association between ministers and transfers

	(1)	(2)
Dependent variable:	$transfers_t$	$transfers_t$
Estimation method:	OLS	sys-GMM
$transfers_{t-1}$	0.175*	0.912***
	(0.0993)	(0.0802)
$log(grossincome)_t$	-4.112***	-0.983**
	(0.448)	(0.456)
$dependants_t$	5.722**	0.915
	(2.322)	(1.155)
$ministers_{t-1}$	**0.0898*** **	**0.125*** **
	(0.0303)	**(0.0347)**
N	300	300
Instruments	-	41
AR(1) p-value	-	0.08
AR(2) p-value	-	0.398
Hansen p-value	-	0.488

District dummies, year dummies, and district-year trends included in all specifications. Robust standard errors clustered at the district level in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

It is well known that the coefficient on the LDV is downwardly biased when estimating using OLS in the presence of fixed effects. This bias might also affect the other coefficient estimates. The specification shown in column (2) takes first differences of the equation in levels to remove the fixed effects, and uses a GMM instrumentation approach to control for the endogeneity of the transformed LDV (but does not yet control for the endogeneity of our main variable of interest *ministers*). Following Arellano and Bond (1991), the 2nd to 4th lag of the levels of the dependent variable are used as instruments for its lagged differences (difference GMM). Simultaneously, differences of the LDV are used as instruments for the equation in levels (system GMM) as in Arellano and Bover (1995) and Blundell and Bond (1998). All estimation was performed using the xtabond2 command in Stata. (see Roodman 2009b, for details)

The results indeed suggest that the coefficient on the LDV was severely downward biased in the OLS estimation. The reported standard tests for autocorrelation allow to reject the presence of first order autocorrelation in the residuals, but not of 2nd order autocorrelation, which matches the assumptions of this estimator. The Hansen over-identification test does not allow to reject the validity of the over-identifying assumptions, but given the large number of instruments, the power of this test is probably low. A robustness-check in section 4.3.3 shows that in specifications with a reduced instrument set, the main conclusions of our analysis remain to hold and the validity of the over-identifying restrictions is upheld.

The association between the one year lag of *ministers* and transfers in year *t* is given by $b_1 \sum_t {b_0}^{t-1}$, with b_1 the effect of the one-year lag of *ministers*, and b_0 the coefficient on the LDV. For large *t*, this effect converges to $b_1/(1-b_0)$. As it turns out, this estimated long run effect is not significantly different from 0 for the estimates reported in column (2). However, in the perhaps more relevant `medium run', of –e.g.- 8 years, or 2 full terms for a minister, the estimated association is about 74(33) euro per capita per year, is significantly different from 0 on the 5-percent level, and is both statistically and economically significantly larger than the estimated 12.5(3.5) euro after the first year. After a single term of 4 years, the estimated association is 44(15) euro.

4.3. Estimation results when instrumenting changes in political representation

Previous contributions (such as Milligan and Smart, 2005, e.g.) do not consider the possible endogeneity of political variables, in particular of provenance of ministers, when explaining geographic redistribution. Milligan and Smart (2005, p. 14) claim that, because in a strong party system like Canada's ministers are appointed by their prime minister, they are exogenous to transfers. A similar reasoning could be applied to a strong party system like Belgium's, where party presidents of the governing coalition appoint ministers. However, one might still argue that *ministers* are endogenous due to reverse causality. It is likely, even in a strong party system, that –as argued above- when a candidate obtains many preference votes during the preceding election, his party president will find it harder to deny this candidate a ministerial post. Obtaining many votes could in turn be due to having channelled extra transfers to one's district. If this is the case, *ministers* would be driving *transfers*, but simultaneously *transfers* in a district would be driving *ministers*. As a matter of fact, it is hard to imagine a mechanism where ministers would channel transfers towards certain districts that cannot be explained by purely socio-economic factors, if not for attracting votes and either directly or indirectly affecting the probability of re-election or re-appointment. To isolate the causal effect of

ministerial representation on transfers toward a district, we therefore will not assume that ministers are appointed exogenously.

As before we will use difference and system GMM estimation to address the bias introduced by the presence of a LDV; but we will now simultaneously instrument the differenced *ministers* variable using changes in *ministers* that may be reasonably thought to be independent from transfers. We develop 2 alternatives to isolate such exogenous minister changes. A first approach is to exploit the resignation of ministers following various events (2 examples are ministers resigning after the escape of a top-criminal and after a food scare), policy disagreements within the ruling coalition (such as a controversial arms deal and an intra-coalition row over airplane noise over Brussels), or appointments of ministers in international institutions. In a 2nd and separate IV analysis, we exploit the changes in political representation that occurred due to the redrawing of electoral district borders in the reform of 2002.

As a robustness-check we perform a 3rd IV analysis where we use the system and difference GMM approach not only for the LDV, but also use this approach for the political representation variable, instrumenting differenced changes in ministers with the lagged level, and vice versa.

4.3.1. Instrumenting using exogenous changes in political representation[25]

Table 7 in Annex 1 gives a detailed description of 12 decreases in *ministers* over our sample period (out of a total of number of 62 changes in *ministers*) that we argue to be exogenous to the transfers received in a region[26]. These 12 cases are due to ministers resigning (or being forced out of) their posts. Most of these 12 resignations correspond to another change in ministerial representation as in most cases the resigning ministers are replaced. However, a minister who enters office exogenously may well be able to start "playing the fiscal transfer game" with a view to obtaining votes during the next elections. We therefore excluded these corresponding positive changes from our analysis. Our results do not change much if we include these corresponding positive changes into the instrument set, however.

None of the 12 considered resignations are prima facie related to federal elections, nor to sub-central or European elections. They mostly comprise hard to foresee cases where ministers have been forced to resign because of some scandal for which they had to take the political responsibility, or chose to step down due to some disagreement within the governing coalition. All 12 do not seem related to the "fiscal transfer game"[27]. Of course, one can never rule out completely that a fiscal-transfer related conflict underlied one or more of the 12 exits concerned, rather than the officially given reason for

[25] We are grateful to Liesbeth Colen for her suggestions regarding the implementation of this IV strategy.

[26] Using a mere subset of the original and possibly endogenous independent variable as the IV is an IV strategy similar to the one implemented in Dahlberg et al. (2008) when investigating the flypaper effect for Swedish municipalities. They use a *census-determined* (i.e. partial) change in grants to instrument the *actual* (i.e. total) change in grants received by Swedish municipalities.

[27] A resignation unrelated to the "fiscal transfer game" that we did not include in table 4 is the appointment of then prime minister Van Rompuy to the post of President of the EU Council of Ministers in 2009. Because of the government reshuffle that followed Van Rompuy's resignation, Van Rompuy's electoral district, Brussel-Halle-Vilvoorde not only lost a minister, but also gained a minister (Vanackere). The net effect of Van Rompuy's resignation on his district's number of ministers hence was zero. Our estimation results are robust to including Van Rompuy's resignation into our set of exogenous minister changes.

their resignation. To the extent that this is the case, our approach will be biased. However, we believe that a fiscal-transfer related conflict did not underlie any of the 12 exits we consider.

Table 2 reports the results of our IV regressions. As before, all specifications use difference and system GMM, with the 2nd to 4th lag of the LDV as instruments in the differenced equation, and first differences values of the LDV as instruments in the level equation.

Table 2: Effect of ministers on transfers, IV estimation.

	(1)	(2)	(3)
Dependent variable:	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	sys-GMM	sys-GMM	sys-GMM
$transfers_{t-1}$	0.931***	0.864***	0.909***
	(0.0844)	(0.108)	(0.0784)
$log(grossincome)_t$	-1.011*	-1.609***	-1.049**
	(0.501)	(0.555)	(0.467)
$dependants_t$	0.993	0.452	0.972
	(1.418)	(1.638)	(1.261)
$ministers_{t-1}$	**0.263****	**0.273*****	**0.184*****
	(0.0938)	**(0.0855)**	**(0.0471)**
N	300	240	300
Instruments	41	39	44
AB-AR(1) p-value	0.097	<0.00	0.081
AB-AR(2) p-value	0.446	0.074	0.41
Hansen p-value	0.997	1	1

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The specification in column (1) uses the subset of decreases in *ministers* that are due to a limited set of assumed exogenous causes (reported in table 7 in Annex 1) as an instrument for the original *ministers* variable. This subset of changes is only used as an instrument for the equation in first differences in the GMM system. The resulting estimated causal effect of *ministers* on *transfers* in column (1) corresponds to a short run effect of 26(9) euro. Despite retaining only 12 out of 62 changes in *ministers* as exogenous to the transfer game, this estimate is both large and significantly different from 0. As the within standard deviation of our ministers variable over sample period is 1.22, the typical variation in transfers per capita over the short term due to variations in our ministers variable over time therefore equals about 32 euro, with a p value of 0.01. The estimated long run effect using this specification is as large as 381(94) euro, and the effect after a single term of 4 years is 95(39) euro. Again, as the within standard deviation of our ministers variable over sample period is 1.22, the typical variation in transfers per capita over the medium term due to variations in our ministers variable over time therefore equals about 116 euro, with a p value of 0.02. The long run and medium run effects are both significantly larger than 0, and significantly larger than the short run effect. These effects are also large when compared to the results obtained in table 1, ignoring the possible endogeneity of *ministers.*

4.3.2. Instrumenting using changes due to electoral districts merger[28]

As a 2nd IV strategy, we exploit changes in political representation caused by a major electoral district reform. As the maps in Annex 3 show, on the occasion of the 2002 reform the former 20 federal electoral districts were reduced to 11. The law concerned merged these 20 districts into 11 larger "provincial" electoral districts[29]. We now instrument *ministers* using as an instrument only the subset of changes in *ministers* that are due to this merger. This instrument is exogenous, if we are willing to assume that voters in the elections of 2003 did not vote with forward looking transfer-related expectations regarding ministers they could additionally vote for due to the electoral district merger, and that ministers did not anticipate the merger and did not steer transfers before the merger to their future new constituency. To the extent that these assumptions fail to hold, our estimate would in part still reflect a reverse causal effect.

Additionally, we have to assume that the district border reform was not implemented for reasons related to fiscal transfers. The official reason given for the district merger is that it would eliminate the arbitrariness of the allocation of the "final" parliamentary seat per province. Under the old district borders, residual votes per district per party would be added up per province, after which some complex process with a rather unpredictable outcome determined which party would obtain the "final" seat. The more probable underlying motivation for the district merger was rather that the politicians –mostly federal ministers- dominating the then 2 largest Flemish governing parties –who masterminded the reform- wished to benefit from the fact that they were well known also beyond the borders of their old district after their parties had governed together since mid-1999. Hence the motivation for the merger was not related to the fiscal transfers that make up our dependent variable, but was rather implemented for reasons related to the country-wide actions of powerful politicians. In contrast, the then major Flemish opposition party heavily resisted the reform. In short, we believe that the underlying reason for the district border reform was unrelated to fiscal issues, or transfers.

Annex 2 describes our instrument over the 2003-2007 period. We assume that our instrument takes values different from zero only over this period, assuming that ministers started at the earliest to steer transfers to their "new districts" in 2003. 2003 is the year of the first elections after the merger law was voted –in September 2002-, so we may assume that the "new" kind of transfers did not arrive in time to influence the election outcome of 2003[30]. We drop all observations after 2007 from the analysis, because 2007 was another federal election year after which ministers could have been appointed (or not) because they had been successful (or not) in steering transfers. Hence after 2007 we cannot safely assume anymore that our instrument is exogenous to transfers.

[28] We are grateful to Matz Dahlberg for suggesting this IV to us in the context of our analysis. More generally, the potential of using redistricting –being orthogonal to voters' preferences- as a way of trying to deal with the endogeneity of political control is suggested by Besley and Case (2003 p. 34).

[29] With the notable exceptions of the old electoral districts of Brussel-Halle-Vilvoorde and of Leuven, which were kept into existence. So after the 2002 reform, Belgium was left with 11 electoral districts, of which 9 truly provincial.

[30] Our regression results reported below are hardly affected if we assume –as a robustness check- that ministers were already appointed in 2002, to reflect that they may already have started steering transfers to "their new districts" from 2002 onwards, anticipating the electoral boundary reform. (In this case, the coefficient on lagged ministers is 0.275(0.107), with a p value of 0.02.) Effectively, this reform was extensively debated in parliament and in the press before it was voted. But it was also quite heavily disputed before it was submitted to parliament, even within the governing coalition. After it was voted in parliament, opposition parties continued opposing it, including in court.

It is clear from Annex 2 that only a limited number of old districts obtained ministers because of the 2002 district merger. These are the 12 old electoral districts belonging to the provinces of Hainaut, Liège, Oost-Vlaanderen and West-Vlaanderen. The boundaries of the other old electoral districts Brussel-Halle-Vilvoorde, Leuven, Limburg, Luxembourg and Namur were left unchanged. The old districts of the province of Antwerp were merged during the reform, but this does not affect our instrument as there was no federal minister originating from these districts during the 2003-2007 period. The last column of Annex 2 gives an overview of the changes in *ministers* due to the border changes. Over the considered time period, the correlation between changes in the *ministers* variable and this subset of changes due to the redrawing of borders contained in the instrument is quite high, with a correlation coefficient of 0.35.

Column (2) of table 2 reports the results of the IV analysis using these changes due to the redrawing of electoral districts as an instrument. The coefficient of *ministers* in column (2) is 27 euros, which is significant at the 0.01 level. Although the specification is similar to the specification in column (1), apart from the shorter sample and the different instrument, the AR(2) test now rejects the absence of an AR(2) process in the residuals, which would invalidate the analysis. We therefore performed a regression where we use not the 2nd to 4th lag of the LDV as instruments, but rather the 3rd to 4th; and also not the lagged changes in political representation but the 2nd lag of those changes. These further lags of the instruments are valid even in the presence of autocorrelation of order 2, if we can exclude autocorrelation of order 3. Indeed, there is little proof of AR(3) in the differenced residuals, as the Arellano-Bond test with a null hypothesis of absence of autocorrelation of order 3 has a p-value of 0.83. We omit the results of this specification as they are very similar to the reported ones, with a coefficient on the LDV of 0.81(0.12), and a coefficient on *minister*s of 0.26(0.12).

We next calculate the effects over time using the formula $b_1 \sum_t {b_0}^{t-1}$, with b_1 the effect of the one-year lag of *ministers*, and b_0 the coefficient on the LDV. For large *t*, this effect converges to $b_1/(1-b_0)$. We obtain an estimated long run effect of 140(97) euro per capita and year; and an effect of 79(36) euro per capita/year after a single term of 4 years.

4.3.3. Extensions and robustness

This section considers several alternative specifications which serve to gauge the robustness of our results.

As a first robustness check, column (3) of table 2 shows the results when instrumenting lagged differenced *ministers* not with an external instrument, but rather using only the past information contained in its own 2nd to 4th lagged levels. The coefficient on *ministers* is significantly different from zero, and smaller than the estimate obtained when using the set of resignations or the border changes as an instrument.

Secondly, as the Hansen IV-overidentification test loses power as the instrument set becomes larger, we re-estimated our main specifications using a restricted instrument set, as shown by table 3. More specifically, we drop the equation in levels, using only the equation in first differences (difference GMM). We drop *dependants* as an explanatory variable (as it is highly co-linear with income and quite often insignificant). We instrument the first-differenced LDV only with the 2nd and 3rd lagged levels (rather than including the 4th). Additionally, we do not include district-year trends but only the full set of year-dummies. This reduces the instrument count considerably in all specifications. The

year-dummies account for 14 of the remaining 18 instruments in the specification in column (1) of table 3. The results in table 3 show that our main results are qualitatively the same despite these many changes in the specification, and that the Hansen tests do not allow to reject the validity of the reduced instrument set.

Table 3: Effect of political representation as measured by IV estimation, with a restricted instrument set

	(1)	(2)	(3)
Dependent variable:	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	diff-GMM	diff-GMM	diff-GMM
$transfers_{t-1}$	0.871***	0.826***	0.861***
	(0.108)	(0.137)	(0.124)
$log(grossincome)_t$	-1.326**	-1.700*	-1.350*
	(0.623)	(0.824)	(0.700)
$ministers_{t-1}$	**0.194****	**0.271*****	**0.164*****
	(0.0683)	**(0.0891)**	**(0.0562)**
N	300	240	300
Instruments	18	16	19
AB-AR(1) p-value	0.079	<0.000	0.067
AB-AR(2) p-value	0.419	0.061	0.393
Hansen p-value	0.273	0.919	0.221

District dummies and year dummies included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Thirdly, as suggested by an anonymous referee, when not normalising *ministers* by the population in each electoral district, the levels of political representation are kept discrete so that the distribution is dominated by a limited set of values. About 47 percent of our district-year observations have 0 ministers, and about 36 percent have 1 minister. To investigate whether the amount of political representation is non-linear, whether our results are driven by just a few outliers with a very high level of political representation, and to test in general whether our results are sensitive to re-defining the measure of political representation, table 4 below shows the result when *not* normalising ministers by population in each district, and when measuring instead the effect on transfers of the variable *[ministers ≥ 1]*, which is a dummy variable taking on a value of 1 when a district has *one or more ministers* and a value of 0 in case the district has *no ministers at all*, rather than a continuous measure. Comparing table 4 shown below with table 2 shows that the results are qualitatively unchanged, although size and significance of coefficients of interest are lower in table 4.

Table 4: Effect of political representation as measured by IV estimation, using a dummy variable for political representation

	(1)	(2)	(3)
Dependent variable:	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	sys-GMM	sys-GMM	sys-GMM
$transfers_{t-1}$	0.977***	0.903***	0.912***
	(0.127)	(0.136)	(0.0852)
$log(grossincome)_t$	-0.953	-1.693**	-1.079**
	(0.613)	(0.672)	(0.483)
$dependants_t$	0.194	-0.0126	0.565
	(1.561)	(1.913)	(1.316)
$[ministers \geq 1]_{t-1}$	**0.0889***	**0.110****	**0.0447****
	(0.0436)	**(0.0463)**	**(0.0203)**
N	300	240	300
Instruments	41	39	19
AB-AR(1) p-value	0.102	<0.000	0.067
AB-AR(2) p-value	0.437	0.065	0.393
Hansen p-value	1	1	0.221

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As a 4th robustness check, we have included so called secretaries of state into our regression analysis below. Secretaries of state are junior members of the government who are in charge of policy areas considered less important. They are not allowed to participate in the weekly meetings of the "Council of Ministers", i.e. the entire cabinet of ministers, which jointly takes the main government decisions. The only way for secretaries of state to influence the decisions of the Council of Ministers is by requesting the minister to whom they are "attached" to make their case. Also, the budget at the disposal of secretaries of state to hire personal advisors so as to staff their private "cabinets" appears considerably smaller than that of federal ministers. Over our sample period, there were on average 15 federal ministers in any single year, and 2 federal secretaries of state.

We created a new variable *ministers and secretaries of state* containing both the ministers and secretaries of state for each district (again normalised by 100,000 inhabitants). Column (1) in table 5 reports the results for the IV analysis using the list of resignations as instruments. 3 additional resignations, by secretaries of state, were included (see table 8 in Annex 1). Column (2) reports the IV analysis using lagged information as instruments. The results reported in table 5 indicate that the association between ministers –including secretaries of state- and transfers becomes of considerably smaller size and significance compared to columns (1) and (3) in table 2, and even turns insignificant when using resignations as instruments. This finding is consistent with the findings of Golden and Picci (2008 p. 286) for Italy.

Table 5: Effect of political representation as measured by IV estimation, including secretaries of state

	(1)	(2)
Dependent variable:	$transfers_t$	$transfers_t$
Estimation method:	sys-GMM	sys-GMM
$transfers_{t-1}$	1.029*	0.846***
	(0.516)	(0.0881)
$log(grossincome)_t$	-0.659	-1.378**
	(2.467)	(0.527)
$dependants_t$	0.728	0.992
	(1.455)	(1.269)
***ministers and secretaries of state*$_{t-1}$**	**0.193**	**0.0915***
	(0.172)	**(0.0464)**
N	300	300
Instruments	41	41
AB-AR(1) p-value	0.079	0.256
AB-AR(2) p-value	0.391	0.575
Hansen p-value	0.646	0.386

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As a last extension, we attempt to estimate a separate effect for predominantly French-speaking districts and Dutch speaking districts. We prefer to make this rather straightforward linguistic-geographic distinction rather than a more complicated distinction, such as between the 3 Belgian regions (Flanders, Wallonia and Brussels), or the 3 Belgian linguistic communities (the Flemish, the Francophone and the German speaking community). This is because at the Belgian *federal* level, which is the level at which we conduct our research, the relevant distinction does not seem to be between the 3 regions or between the 3 communities, but between the 2 language groups in the federal parliament. By law, every federally elected politician belongs to either the Dutch-speaking or to the French-speaking language group. The Dutch-speaking language group consists 100% of politicians elected on lists submitted by Flemish parties, and the French-speaking language group consists 100% of politicians elected on lists submitted by Francophone parties. In practice, this implies that over our sample period every single district only "supplied" members of parliament either to the Dutch-speaking or to the French-speaking group in the federal parliament, with the exception of the district of BHV[31].

Hence, we opted to split only between the French-speaking and Dutch-speaking ministers. To this effect, we created a dummy *Francophone* taking the value of 1 for a French-speaking district, and added this new variable as well as its interaction with the original *ministers* variable labeled

[31] However, we have considered the district of BHV as Francophone, as more voters in this district vote for Francophone parties than for Flemish parties.
Apart from our belief that the language groups are the most relevant way to split our sample, we have to note that our sample size is limited. We would never be able to estimate, say, the effect of the single member of parliament originating from the very small German speaking community, with a population of around 76,000. It also would be hard to differentiate between languages and regions simultaneously, given the large overlap.

*ministers*Francophone*, as regressors to our analysis. Table 6 shows the results of such an inclusion. See Brambor et al. (2005) for the interpretation of the coefficients in such interaction models.

The results of this exercise are rather inconclusive. The first IV in column (1) which uses the list of presumed exogenous resignations as an instrument, appears to indicate a high effect of representation in Dutch speaking districts, and an effect in French-speaking districts which may be somewhat lower. But the point estimate of the difference of the effect between the 2 types of districts of -0.0632 is estimated very imprecisely. As a result, the point estimate of the effect in a Francophone district of 0.199(0.16) is not significantly different from 0. In the 2nd IV analysis using the border change (column (2), both effects are estimated very imprecisely, and there is again very weak evidence for the effect in French speaking districts to be lower. The point estimate of an additional minister in a Francophone district is 0.053(0.082) in this specification which is insignificantly different from 0. In the last IV analysis using lagged information on political representation as an instrument, the effects are again very imprecisely estimated. But here, if anything, there is some weak evidence for the effect in French speaking districts to be higher (the interaction effect has a p-value of 0.164). The estimated effect of an additional minister in Francophone districts in this specification is 0.23(0.06) and highly significantly different from 0. In all, there does not seem to be strong evidence suggesting that the results we obtain in our main analysis are driven by just the electoral districts of one of both main language groups in the country.

Table 6: Effect of political representation as measured by IV estimation, estimating separate effects for dominantly Dutch and French speaking districts

	(1)	(2)	(3)
	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	sys-GMM	sys-GMM	sys-GMM
$transfers_{t-1}$	0.929***	0.851***	0.888***
	(0.0833)	(0.121)	(0.0733)
$log(grossincome)_t$	-0.995**	-1.782**	-1.139**
	(0.475)	(0.662)	(0.446)
$dependants_t$	1.195	0.750	0.545
	(1.277)	(2.052)	(1.454)
$ministers_{t-1}$	**0.262*** **	**0.0807**	**0.101**
	(0.0866)	**(0.0530)**	**(0.0613)**
$ministers*Francophone_{t-1}$	**-0.0632**	**-0.0272**	**0.128**
	(0.207)	**(0.117)**	**(0.0888)**
N	300	240	300
Instruments	43	40	48
AB-AR(1) p-value	0.09	<0.000	0.072
AB-AR(2) p-value	0.439	0.068	0.395
Hansen p-value	0.280	1	1

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In all our specifications, it is noticeable that the IV estimates are larger than the OLS estimates. This suggests a downward bias in OLS, rather than an upward bias, which runs counter to our intuition

about the effect of transfers on political representation (through improving re-election chances). The effect might be purely econometrical, however, if the IV estimates remove some measurement error in the original independent variable. Another possibility is that there would be a difference in how some types of political representation have a stronger effect on transfers. One can imagine e.g. that ministers who suddenly need to compete for votes within a substantially enlarged electoral district (see maps in Annex 3) have a stronger incentive to channel transfers towards their constituencies, in order to secure visibility with the local electorate before the next elections. These effects may dominate the removal of the bias through reverse causality, which might be small even in the OLS analysis as the variation over time in *transfers* is small compared to the variation in *ministers.*

5. Conclusion

Controlling for key socio-economic variables such as the local share of dependants in the population and average gross income, we find that after a single term of 4 years, per capita cash transfers to a Belgian electoral district are significantly higher for every federal minister originating from that electoral district. The observed correlation between ministerial representation and transfers might result from a mechanism where ministers cater for their constituency in the form of transfers and are in turn rewarded by voters.

In this paper, we focussed on estimating the causal effect of exogenous changes in ministerial representation on transfers, using 2 IV strategies. The point estimates after a single term of 4 years of 95(39) euro for the instrument based on resignations and of 79(36) euro for the instrument based on border changes –resp. derived from columns (1) and (2) in table 2- appear large when compared to the estimates assuming all changes in political representation are exogenous. Although the estimates differ considerably between different specifications and robustness checks, the results are broadly supportive of the hypothesis that ministers steer transfers to their constituency.

Our findings contribute to the literature on distributive politics in several ways.

Firstly, regarding the type of transfers we consider, virtually the entire empirical distributive politics literature appears to deal with "pork barrel", not with -supposedly- formula-based fiscal transfers to citizens, which make up the core of income redistribution in most developed countries. Our setup and findings can be juxtaposed to the setup of Dixit and Londregan (1998 p. 512) who assume that income taxes would rather serve ideological purposes and targeted handouts would serve tactical purposes; or Milesi-Ferretti et al. (2002 p. 647-648) who assume that public purchases of goods and services are easier to target geographically compared to transfers which are easier to target across social groups. By providing quantitative evidence of political influence in formula-based income redistribution, we provide support to qualitative claims such as by Besley and Coate (2003 p. 2628) who state that "even when spending is allocated formulaically, it is possible for legislators to manipulate such formulas to favour their own districts".

We only know of few studies that demonstrate Besley and Coate's claim empirically. Moreover, our study is the only one of those that explains the distribution of formula-based expenditure and taxation directed at citizens –rather than directed at lower-level governments- over electoral districts by the provenance of cabinet ministers.

A number of questions remain unanswered. A first one is the role of political parties with respect to our minister effect. Political parties are considered to be strong in Belgium. Does our observed minister effect mean that Belgian political parties reward their ministers with extra transfers for their districts in return for services rendered by these ministers to their party while holding office[32]? Or does it mean that the non-re-election of a minister –or a poor election result of a minister- would bring reputational and/or other damage to the party as a whole? Crampton (2004 p. 6) finds higher grants for Canadian home districts of a Cabinet Minister compared to core districts of the governing party. Alternatively, the fact that ministers are able to steer transfers could point at tactical behaviour of ministers towards their party, i.e. at a principal-agent problem between political parties (the principal) and ministers (the agents) going on in Belgium. This would be a similar conclusion to the one drawn by Golden and Picci (2008 p. 286): a failure of the governing political parties to discipline their ministers.

Another remaining question is whether the minister effect is present across the board, i.e. including for ministers not having social security nor income taxation under their remit. We argued that ministers might pressure their colleagues in the council of ministers, or within their party, as an explanation of this across the board effect. But our results do not offer any direct proof of such a mechanism and further research is required in this area. It would be interesting to consider whether some ministers have more influence compared to others, or whether ministers affect transfers more within their own domain.

A final question that we left open is which channels ministers use to tilt taxing & spending in favour of their electoral district. Do they design specific rules or rather bend their implementation? While we suggested possible mechanisms in the introduction of this paper, our regression analysis does not provide evidence in favour of nor against one or the other. Moreover, at first sight Belgian federal cabinet ministers on average do not seem to remain in office long enough –only for 5 years in a row[33]- to be able to make their district benefit from changes that they would have initiated in the "formulae" that underlie the income taxation and the social security system. Another challenge for such a line of research is that one would be looking for mechanisms that are likely being kept hidden intentionally.

References

Albouy, D., "Partisan representation in Congress and the geographic distribution of federal funds", Review of Economics and Statistics 95.1 (2013): 127-141.

Alesina, A., "The political economy of budget deficits". NBER working paper #4637, 1994.

Ansolabehere, S., A. Gerber and J. Snyder, "Equal votes, equal money: Court-ordered redistricting and public expenditures in the American states", American Political Science Review 96.4 (2002): 767-777.

[32] Engstrom and Vanberg (2010 p. 963) follow a similar reasoning for US party leaders with respect to rewarding powerful members of Congress belonging to their party with earmarks to the latter's districts.

[33] To calculate the average term of a Belgian federal minister we excluded the year 1995, which is the first year of our sample period but which was the final year of a legislature. There were 60 minister changes over 300 observations from 1996 until 2010.

Ansolabehere, S., J. Snyder, and M. Ting, “Bargaining in bicameral legislatures: When and why does malapportionment matter?”, American Political Science Review 97.3 (2003): 471-481.

Arellano, M., and S. Bond. "Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations." The Review of Economic Studies 58.2 (1991): 277-297.

Arellano, M., and O. Bover. "Another look at the instrumental variable estimation of error-components models." Journal of econometrics 68.1 (1995): 29-51.

Atlas, C., T. Gilligan, R. Hendershott, and M. Zupan, “Slicing the federal government net spending pie: Who wins, who loses and why?”, American Economic Review, 85:3 (1995): 624-629.

Banful, A. “Do formula-based intergovernmental transfer mechanisms eliminate politically motivated targeting? Evidence from Ghana”, Journal of development economics 96.2 (2011): 380-390.

Besley, T. and A. Case, “Political institutions and policy choices”, Joural of Economic Literature 41 (2003): 7-73.

Besley, T. and S. Coate, “Centralized versus decentralized provision of local public goods: A political economy approach”, Journal of Public Economics 87.12 (2003): 2611-2637.

Bickers, K. and R. Stein, “The electoral dynamics of the federal pork barrel”, American Journal of Political Science 40.4 (1996): 1300-1326.

Blundell, R., and S. Bond. "Initial conditions and moment restrictions in dynamic panel data models", Journal of econometrics 87.1 (1998): 115-143.

Brambor, T., W.R. Clark and M. Golder, "Understanding Interaction Models: Improving Empirical Analyses", Political Analysis 14.1 (2005): 63-82.

Brollo, F. and T. Nannicini, “Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil”, American Political Science Review, 106:04 (2012): 742-761.

Carozzi, F. and L. Repetto, “Sending the Pork Home: Birth Town Bias in Transfers to Italian Municipalities”, CEMFI unpublished paper, 2013.

Crampton, E., “Distributive politics in a strong party system: Evidence from Canadian job grant programs”. University of Canterbury. Unpublished paper, 2004.

Dahlberg, M. and E. Johansson, “On the vote-purchasing behavior of incumbent governments”, American Political Science Review 96 (2002): 27-40.

Dahlberg, M. et al., “Using a discontinuous grant rule to identify the effect of grants on local taxes and spending”, Journal of Public Economics 92 (2008) 2320-2335.

Deloitte, “Studie over de gelijke behandeling van rechthebbenden, werkgevers en belastingplichtigen”. Staatssecretariaat voor de Coördinatie van de Fraudebestrijding. Unpublished paper, 2010.

Dewachter, W., “De mythe van de parlementaire democratie: een Belgische analyse”. Acco, 2001.

Dixit, A. and J. Londregan, “Ideology, tactics and efficiency in redistributive politics”, Quarterly Journal of Economics 113.2 (1998): 497-529.

Dragu T. and Rodden J., “Representation and redistribution in federations”, Proceedings of the National Academy of Sciences (2011): 1-4.

Engstrom, E. and G. Vanberg, “Assessing the allocation of pork. Evidence from Congressional earmarks”, American Politics Research 38.6 (2010): 959:985.

Evans, T., “The impact of representation per capita on the distribution of federal spending and income taxes”, Canadian Journal of Political Science 38.2 (2005): 263-285.

Golden, M. and L. Picci, “Pork-barrel politics in postwar Italy, 1953-94”, American Journal of Political Science 52.2 (2008): 268-289.

Guccio C. and I. Mazza, “On the political determinants of the allocation of funds to heritage authorities”, European Journal of Political Economy 34 (2014): 18-38.

Helland, L. and R. Sørensen, “Geographic redistribution with disproportional representation: a politico-economic model of Norwegian road projects”, Public choice 139 (2009): 5-19.

Hooghe, M., B. Maddens and J. Noppe, “Why parties adapt : electoral reform, party finance and party strategy in Belgium”, Electoral Studies 25 (2006): 351-368.

Jutras, M. and F. Vaillancourt, “Mesure et déterminants des flux financiers fédéraux vers les provinces canadiennes, 1966-2004». Université de Montréal. Unpublished paper, 2008.

Knight, B., “Estimating the value of proposal power”, American Economic Review 95 (2005): 1639-1652.

Knight, B., « Legislative representation, bargaining power and the distribution of federal funds : Evidence from the US Congress », Economic Journal 118 (2008) : 1785-1803.

Larcinese, V., L. Rizzo, and C. Testa, “Allocating the US federal budget to the states: the impact of the president”, Journal of Politics 68 (2006): 447-456.

Levitt, S. and J. Snyder, “Political parties and the distribution of federal outlays”, American Journal of Political Science 39.4 (1995): 958-80.

Litschig, S., “Are rules-based government programs shielded from special-interest politics? Evidence from revenue sharing transfers in Brazil”, Journal of Public Economics, 91:11-12(2012): 1047-1060.

Migueis, M., “The effect of political alignment on transfers to Portuguese municipalities”, Economics & Politics 25 (2013):110-133.

Milesi-Ferretti, G., R. Perotti, and M. Rostagno (2002) “Electoral systems and public spending”, Quarterly Journal of Economics 117.2 (2002): 609-657.

Milligan, K. and M. Smart, “Regional grants as pork barrel politics”, CESifo working paper no. 1453, 2005.

NBB (several issues), “Regionale rekeningen”.

Padovano, F., “The drivers of interregional policy choices: evidence from Italy”, European Journal of Political Economy, 28(2012): 324-340.

Persyn, D. and K. Algoed, "Interregional redistribution, growth and convergence", Vives discussion paper 4, 2011.

Rodden, J. and S. Wilkinson, “The shifting political economy of redistribution in the Indian Federation”. Preliminary draft, 2004.

Roodman, D., "A note on the theme of too many instruments", Oxford Bulletin of Economics and Statistics 71.1 (2009a) 135–158.

Roodman, D., “How to do xtabond2: An introduction to difference and system GMM in Stata,” Stata Journal 9.1 (2009b) 86-136

Sorribas-Navarro, P., “Bailouts in a federal system: evidence from Spain”, European Journal of Political Economy 27 (2011): 154-170.

Stratmann, T., “The effects of earmarks and the likelihood of reelection”, European Journal of Political Economy 32 (2013): 341-355.

Timmermans, A., “Cabinet ministers and coalition formation in Belgium: the impact of coalition constraints”, in: Laver, M. and K. Shepsle (eds.), “Cabinet ministers and parliamentary government”. Cambridge University Press, 1994.

Young, A. and R. Sobel, “Recovery and Reinvestment Act spending at the state level: Keynesian stimulus or distributive politics?”, Public Choice 155 (2013): 449-468.

Wooldridge, J., “Econometric analysis of cross section and panel data”. The MIT Press, 2002.

Annex 1: Districts having lost a minister / secretary of state over our sample period due to his/her resignation for a reason not related to "the fiscal transfer game"

Table 7: Districts having lost a minister over our sample period due to his/her resignation for a reason not related to "the fiscal transfer game"

Year	Electoral district	Minister	Exogenous reason
1996	Verviers	Wathelet Sr	Appointed judge at European Court of Justice
1998	Kortrijk-Roeselare-Tielt	Declerck	Resigned after escape of serial murderer and child rapist
1998	Veurne-Diksmuide-Ieper-Oostende	Vandelanotte	Resigned after escape of serial murderer and child rapist
1999	Antwerpen	Colla	Resigned after outbreak of food scare
1999	Limburg	Pinxten	Resigned after outbreak of food scare
2003	Brussel-Halle-Vilvoorde	Durant	Resigned after coalition conflict about airplane noise over Brussels
2003	Leuven	Aelvoet	Resigned after coalition conflict about arms deal with Nepal
2009	Aalst-Oudenaarde	De Padt	Demoted by his party to the position of "government commissioner" without a reason being known
2009	Limburg	Dewael	Resigned after allegations of influencing a civil service appointment
2009	Limburg	Vandeurzen	Resigned after the collapse of a large Belgian bank (Fortis)
2009	Mechelen-Turnhout	Vervotte	Resigned after the collapse of a large Belgian bank (Fortis)
2009	Veurne-Diksmuide-Ieper-Oostende	Leterme	Resigned after the collapse of a large Belgian bank (Fortis)

Source: Wikipedia; Federal Parliament of Belgium

Table 8: Districts having lost a secretary of state over our sample period due to his/her resignation for a reason not related to "the fiscal transfer game"

Year	Electoral district	Secretary of state	Exogenous reason
2001	Brugge	Chevalier	Resigned after charges by a Swiss court with respect to a financial crime
2003	Brussel-Halle-Vilvoorde	Deleuze	Resigned after coalition conflict about airplane noise over Brussels
2008	Mons-Soignies	Donfut	Resigned after having been paid for consultancy to a local government body

Source: Federal Parliament of Belgium

Annex 2: Number of extra ministers per district due to district merger over our sample period

Year	New electoral district	Old electoral district	Extra ministers due to merger	Population share of old in new district	Weighted extra ministers due to merger
2003	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2003	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	3	0.30	0.90
2003	Oost-Vlaanderen	Aalst-Oudenaarde	4	0.28	1.10
2003	West-Vlaanderen	Brugge	1	0.24	0.24
2003	West-Vlaanderen	Kortrijk-Roeselare-Tielt	1	0.45	0.45
2003	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	0	0.31	0.00
2003	Hainaut	Mons-Soignies	0	0.33	0.00
2003	Hainaut	Tournai-Ath-Mouscron	0	0.23	0.00
2003	Hainaut	Charleroi-Thuin	0	0.44	0.00
2003	Liège	Liège	1	0.57	0.57
2003	Liège	Huy-Waremme	2	0.17	0.34
2003	Liège	Verviers	3	0.26	0.79
2004	Oost-Vlaanderen	Gent-Eeklo	0	0.42	0.00
2004	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	3	0.30	0.90
2004	Oost-Vlaanderen	Aalst-Oudenaarde	3	0.27	0.82
2004	West-Vlaanderen	Brugge	1	0.24	0.24
2004	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2004	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	1	0.31	0.31
2004	Hainaut	Mons-Soignies	2	0.33	0.66
2004	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2004	Hainaut	Charleroi-Thuin	1	0.44	0.44
2004	Liège	Liège	1	0.57	0.57
2004	Liège	Huy-Waremme	2	0.17	0.34
2004	Liège	Verviers	3	0.26	0.79
2005	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2005	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	2	0.30	0.60
2005	Oost-Vlaanderen	Aalst-Oudenaarde	3	0.27	0.82
2005	West-Vlaanderen	Brugge	1	0.24	0.24
2005	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2005	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	1	0.31	0.31
2005	Hainaut	Mons-Soignies	2	0.33	0.66
2005	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2005	Hainaut	Charleroi-Thuin	1	0.44	0.44
2005	Liège	Liège	1	0.57	0.57
2005	Liège	Huy-Waremme	1	0.17	0.17
2005	Liège	Verviers	2	0.26	0.52
2006	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2006	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	2	0.30	0.60
2006	Oost-Vlaanderen	Aalst-Oudenaarde	3	0.27	0.82
2006	West-Vlaanderen	Brugge	0	0.24	0.00
2006	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2006	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	2	0.31	0.63
2006	Hainaut	Mons-Soignies	2	0.33	0.66
2006	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2006	Hainaut	Charleroi-Thuin	1	0.44	0.44
2006	Liège	Liège	1	0.57	0.57
2006	Liège	Huy-Waremme	1	0.17	0.17
2006	Liège	Verviers	2	0.26	0.53
2007	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2007	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	1	0.30	0.30
2007	Oost-Vlaanderen	Aalst-Oudenaarde	2	0.28	0.55
2007	West-Vlaanderen	Brugge	0	0.24	0.00
2007	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2007	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	2	0.32	0.63
2007	Hainaut	Mons-Soignies	2	0.33	0.66
2007	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2007	Hainaut	Charleroi-Thuin	1	0.44	0.44
2007	Liège	Liège	1	0.57	0.57
2007	Liège	Huy-Waremme	1	0.17	0.17
2007	Liège	Verviers	2	0.26	0.52

Source: Federal Parliament of Belgium

Annex 3: Federal electoral districts before and after the 2002 reform

Source: Hooghe e.a. (2006)

Chapter 2: Interregional net fiscal transfers resulting from the Belgian federal public debt: political economy and proposal for a calculation[34]

Abstract

We calculate that over the 1970-2002 period the combination of persistent primary surpluses of the Flemish region and persistent primary deficits of the Walloon region –and to a lesser extent of the Brussels region-, all implicit in the *Federal government* budget of Belgium, increased the net fiscal transfers due to the federal debt provided by Flanders to Francophone Belgium to no less than 5.6% of Belgian GDP or almost 100% of the total interest bill on the Belgian federal debt. This amount is considerably larger than the net transfers amount resulting from previous traditional interregional net transfers calculations, which implicitly assume a balanced federal budget. The geographically very unevenly spread benefits from Belgian federal budget deficits –and hence the geographically strongly diverging incentives for federal budget deficits- may well explain to a considerable extent the large overall size of deficits and debt in Belgium since the 1970s. In other words, interregional net fiscal transfers resulting from the federal debt may be important not just because of their distributional *consequences*, but even more because they may provide an *explanation* for the large overall size of deficits and debt that have particularly characterised Belgium since the 1970s.

The geographically apparently strongly diverging incentives for federal budget deficits in Belgium add a new dimension to traditional political economy motives for debt financing. Persson and Tabellini's (2000) *partisan* dimension *over time* to political economy motives for debt financing takes the shape of a government party with more or less polarised preferences compared to the opposition's, and of governments more or less quickly alternating over time. While such political instability may be viewed as polarisation that exists *between governing and opposition parties*, the additional phenomenon that seems to particularly apply to the Belgian federal level is fragmentation, to be viewed as polarisation *within the governing coalition itself*. In contrast to instability, fragmentation adds a *simultaneous partisan* dimension to political economy motives for financing in Belgium. What is more, Belgian political parties are linguistically and hence largely *geographically* split. Such a split once again increases the number and probability of veto rights over fiscal policy. The additional potential consequences of the Belgian *simultaneous geographic* dimension have hardly been investigated in the existing literature on the political economy of budget deficits. A challenge for Belgium seems to be to explain the *overall size* of deficits and debt by the *geographically unevenly spread costs and benefits* stemming from deficits and debt.

Keywords: distributive politics; income redistribution; fiscal transfers; public debt; elections; fiscal federalism; politicians; political institutions

[34] This paper is a substantially reworked and extended version of earlier papers on the same subject, in particular of "Transferten tussen de Belgische gewesten uit intrestlasten op de federale overheidsschuld" (2012). It is in the process of being published in Kyklos. It is to an important extent based on earlier work by Koen Algoed (2009) and Johan Van Gompel (2004). I also thank Thushyanthan Baskaran, Richard Bird, Erik Buyst, Stefaan Decramer, Herman Deweerdt, Danny Geerts, Joep Konings, Eric Ponette, Willem Sas, Klaas Staal, David Stadelmann, François Vaillancourt and Tom Van Puyenbroeck for comments on current and/or earlier versions of this paper.

1. Introduction

Over the past decades, not many OECD countries combined a high debt burden and a large welfare state with sizeable interregional disparities in tax revenues and public spending (which in turn may be due to interregional disparities in gross income and/or political representation). Examples of such countries are Belgium and Italy, which over the past decades combined a high public debt with sizeable net fiscal transfers between their northern and southern regions.

For the year 2005 –taken as an example-, and for those OECD countries only that suffered from a debt ratio that was higher than the OECD average, table 1 shows total government spending and total social spending as a % of GDP, as well as the Gini coefficient and the Coefficient of Variation (CV) of GDP per capita between regions. We suspect that countries combining a high debt ratio, high public spending –in particular social spending-, and high interregional income disparities also suffer from high interregional net transfers stemming from the servicing of the public debt. This is because we would expect high income disparities to generate large interregional net transfers due to social spending. Table 1 shows that among OECD-countries only Belgium, Greece, and Italy *simultaneously* score higher than average on total government spending, on total social spending, and on interregional income disparity[35], while suffering –together with Japan- from the largest public debts among OECD-countries.

While we will find in what follows that the existing political economy literature on deficits and debt appears to explain the overall size of deficits and debt *purely* by *non-geographic* political economy factors, we will calculate that the net benefits from federal budget deficits in Belgium are geographically very unevenly spread. This geographic disparity may create geographically strongly diverging incentives for federal budget deficits, and hence may well explain to a considerable extent the large overall size of deficits and debt in Belgium since the 1970s. In other words, interregional net fiscal transfers resulting from the federal debt may be important not just because of their distributional *consequences*, but even more because they may provide an *explanation* for the large overall size of deficits and debt that have particularly characterised Belgium since the 1970s. The implications seem similar for politicians from other countries with a high central government debt and considerable disparities in the extent to which its resp. regions provide tax revenues to the central government, resp. benefit from central government spending, such as Italy and Greece.

The remainder of the paper is composed as follows. First we will situate Belgium –a country that combines a high public debt with a large welfare state and high geographic income disparities- within the existing theoretical and empirical literature on the political economics of debt. Also we will adapt the existing theoretical literature to incorporate the geographic dimension to the political economics of debt that we suspect to exist in countries like Belgium. In a next chapter, we will propose a methodology for the calculation of interregional net fiscal transfers resulting from the public debt, and apply this methodology to Belgium. A final chapter will conclude.

[35] Belgium, Greece and Italy also show the highest interregional disparity in terms of *GDP per capita* among EU15-member states (measured by the Gini coefficient and by the CV), except France (measured by CV) and Ireland (measured by Gini).

Table 1: Central government debt, size of government and social spending as a % of GDP, as well as Gini and CV coefficients for GDP per capita between regions for selected OECD countries (only those with a higher than average debt ratio) (2005)

	Central govt debt (% of GDP)	General govt expenditures (% of GDP)	Social expenditures (% of GDP)	Gini of GDP per capita*	CV of GDP per capita*
Austria	62.1	49.5	27.1	0.13	0.21
Belgium	**91.8**	**49.8**	**26.5**	**0.34**	**0.34**
France	53.3	53.3	30.1	0.07	0.31
Greece	**110.6**	**50.6**	**21.1**	**0.16**	**0.29**
Hungary	58.1	49.2	22.5	0.21	0.48
Israel	92.1	43.1	16.3		
Italy	**97.7**	**48.6**	**24.9**	0.14	**0.27**
Japan	**164.5**	36.9	18.6	0.07	0.11
Poland	44.8	43.2	21.0	0.12	0.30
Portugal	66.2	44.8	23.0	0.13	0.25
Sweden	46.2	51.8	29.1	0.08	0.22
Turkey	51.1		9.9	0.24	0.45
OECD average	**44.2**	**40.9**	**19.7**	**0.15**	**0.25**

*between NUTS1 regions[36]

Note: CV = coefficient of variation

Source: OECD website; OECD 2009 p. 28

2. The political economics of public debt and its application to Belgium

First we will give an overview of the existing literature on the political economics of public debt. We will distinguish between the literature with respect to the debt game *between successive governments* and the literature with respect to the debt game *within the same government.* Next we will check the extent to which these 2 literatures hold for Belgium. At least, there will seem to be a dimension missing to the existing literature with a view to making a political economics analysis of the debt game in Belgium. Therefore we will conclude this section with an analysis of the Belgian debt game as a game between *different regions* represented in the *same* central government.

2.1. The existing literature on the political economics of public debt

2.1.1. The debt game *between successive governments*

An established theoretical finding in political economics is that a political party in power benefits a priori from a budget deficit, along the line of reasoning of Persson and Tabellini (2000 p. 345-372),

[36] NUTS1 is the highest level of geographical disaggregation of EU member states for statistical purposes by Eurostat.

with additional reference to Lizzeri (1999 p. 909-912) and Besley (2006 p. 210-211). This is because in the *current* legislature, an incumbent political party should expect to benefit electorally from an expenditure increase. This is the traditional common pool problem: taxation is general while expenditures can be targeted. But this benefit is even larger if the incumbent political party avoids to finance the expenditure increase with a tax increase during the same legislature. This is another kind of common pool problem, i.e. not between different taxpayers during one and the same legislature, but between taxpayers in the current legislature and taxpayers in future legislatures.

In Persson and Tabellini (2000 p. 345)'s wording: poor definition of property rights to *both current and future* tax revenues (or non-internalisation of the cost of borrowing in terms of future spending cuts or tax increases) gives the incumbent parties not only an incentive "to spend *a lot*" but also "to spend *soon*". Otherwise stated: debt financing arises because an incumbent party is generally *not certain* to be part of the governing coalition during the *next* legislature[37]. This uncertainty –to be understood as the expectation that power alternates *randomly* between a party in power and its competitor(s)- not only gives the party in power an incentive for -re-election improving- deficit spending *now*, but it also means that it may be exempted from bearing the *future* costs of current deficit spending[38].

Being in power even without running a deficit already enables a party to favour its constituency with extra expenditures. But debt financing *increases* the room for such *current* extra expenditures to be targeted to a party's constituency. Moreover, at the same time it limits the room for *future* extra expenditures to be targeted to a *competitor party*'s constituency (it "ties its hands"), in case a competitor party gets elected instead of the incumbent. (Alesina and Perotti 1994 p. 17) This asymmetry between the costs and benefits of deficits may be an additional reason why, within any coalition government, those coalition members wishing to *prevent* a budget deficit –which could be those coalition members that can be rather sure of being part of the next government- may face a more difficult task than those coalition members wishing to *elicit* a debt-funded expenditure increase –which could be those coalition members that are less sure of being part of the next government.

Pettersson-Lidbom (2001) tests the theoretical finding of Persson and Tabellini (2000) for Swedish municipalities. He finds that a rightwing party that is certain of being voted out of a Swedish municipal government *increases* debt with 15% compared to when it is certain of being voted again into government, but that a leftwing party *decreases* debt with 11% in such a situation. Pettersson-Lidbom does not explain this difference in behaviour between leftwing and rightwing parties, but in an earlier paper, Persson and Svensson (1989) had established the theoretical result that a rightwing party uncertain of re-election increases debt to prevent a leftwing successor to implement its spending priorities ("tying its hands").

2.1.2. The debt game *within the same government*

Persson and Tabellini's *partisan* dimension *over time* to political economy motives for debt financing takes the shape of a –typically single- government party with more or less polarised preferences compared to the opposition's, and with governments more or less quickly alternating over time. This

[37] In fact, Alesina and Tabellini (1987) already came to this result.
[38] Persson and Tabellini (2000 p. 367) note that uncertainty of re-election is higher for (individual parties that are a member of) coalition governments than for single party governments.

dimension may be called political *instability*. However, there could also be a *simultaneous* partisan dimension to political economy motives for debt financing. This dimension typically takes the shape of more or less *internally* fragmented governments at any given point in time. This dimension may be called political *fragmentation*. While instability may be viewed as polarisation that exists *between governing and opposition parties*, fragmentation may be viewed as polarisation that exists *within the governing coalition itself*.

This latter dimension is noted by Alesina and Tabellini (1992 p. 342), Alesina and Perotti (1994 p. 22-26), and Persson and Tabellini (2000). Velasco (1997) establishes the theoretical result that the larger the degree of fragmentation in policymaking –i.e. the larger the number of interest groups represented within a single coalition-, the greater the deficit bias. Roubini and Sachs (1989) view avoiding deficits within one and the same government as a prisoner's dilemma: all parties may prefer a balanced budget, but in the absence of strong coordination between them to achieve such a cooperative outcome, each party may have an incentive to protect and expand "its" part of the budget[39]. Additionally, while cooperation of *all* coalition partners may be needed to *avoid* a deficit, non-cooperation by *one* of them may suffice to *cause* a deficit[40].

Roubini and Sachs (1989 p. 909) are the first to empirically analyse political fragmentation. They in particular demonstrate that large, short-lived and uncohesive coalition governments lead to large deficits. While de Haan and Sturm (1997) replicate Roubini and Sachs' (1989) research, and fail to find a relationship between government fragmentation and budget deficits, Spolaore's (1993) research confirms Roubini and Sachs' (1989) finding: the higher the number of parties in a coalition, the larger the deficit in response to a negative economic shock. Another confirmation is Ashworth e.a. (2005), who find for Flemish municipalities that governments comprised of more parties, as well as of ideologically more heterogeneous parties, lead to higher debts. For US states, Alt and Lowry (1994) find deficits more likely under divided government –i.e. with the governor, the House of Representatives' majority, and the Senate's majority not all 3 belonging to the same party. Volkerink and de Haan (2001) and Perotti and Kontopoulos (2002) provide empirical evidence that the number of *ministers* cause the budget deficit to increase more strongly and significantly than the number of *parties* in a coalition[41].

---

[39] Roubini and Sachs (1989 p. 909) additionally point at the *relationship* between the 2 strands in the literature: the higher the uncertainty of re-election of a governing coalition, the higher polarisation and fragmentation *within* the governing coalition. They note from game theory that cooperation is easier to maintain if the expected time horizon over which agents interact is longer.

[40] While Roubini and Sachs (1989) analyse the political economy drivers of debt financing by *central governments*, many more studies are available on the political economy drivers of debt financing of *lower level governments*. The prisoner's dilemma that applies to parties that are member of one and the same central government coalition also seems to apply to states within federations. Rodden (2005 p. 181-221) finds for German states characterised by partisan alignment with CDU and/or SPD governing at the federal level over the 1978-1996 period that they ran higher deficits, and expected to receive –explicit or implicit- bailout grants as a consequence of these deficits. Similarly, Khemani (2007) finds for Indian states characterised by partisan alignment with the leading party in the central government over the 1972-1998 period that they ran higher deficits. A more common and better known finding of empirical research than the effect of *partisan alignment* on lower level government deficits is that grants-based fiscal federalism –and more generally the *availability of grants financing* by the central government as such- leads to higher deficits of lower level governments, again anticipating –explicit or implicit- bailout grants. (see Pettersson-Lidbom 2010 for Sweden; Baskaran 2012 for Germany; and Sorribas-Navarro 2011 for Spain)

[41] Likewise, Jennes (2015) finds for Belgian supra-local governments that their deficits increase in their number of ministers. Perotti and Kontopoulos (2002 p. 213-214) find that the number of ministers and the number of parties are an even stronger determinant of the budget deficit in times of economic crisis ("difficult times").

2.2. The existing literature on the political economics of public debt applied to Belgium

2.2.1. The debt game *between successive governments* applied to Belgium

Among those 6 parties that all but monopolised the holding of federal office amongst each other over our period studied -1970-2002, the parties that were least sure of being part of the next government were the Flemish and Francophone liberal-conservative parties. Hence the liberal-conservative parties seemed to have had the greatest interest in debt accumulation during their coalition participation. We measure uncertainty of being part of the next Belgian Federal government as the number of years during which a particular party was *previously* in opposition, divided by the total number of years during which a particular party was *previously* in opposition or in power[42]. In terms of our "power uncertainty" measure, the liberal-conservatives were followed rather closely by the socialists, and followed at a large distance by the Christian-democrats. This ranking both holds when we take into account all Belgian central governments either from the introduction of the universal suffrage in 1918 onwards, or from the end of WWII onwards, or from the start of our sample period onwards. (see Luykx 1985 and Res Publica) However, the uncertainty gap between the liberal-conservatives and the socialists widens over time, while the uncertainty gap between the socialists and the Christian-democrats reduces over time.

The seemingly strong interest in debt accumulation of liberal-conservative parties is a paradox as rather rightwing parties are mostly associated with fiscal discipline, but it is a result that –as said above- has been found theoretically by Persson and Svensson (1989) and by Pettersson-Lidbom (2001) empirically. Over our period studied -1970-2002- the liberal-conservative parties were *only* part of the federal coalition during the 1974-77, 1981-87, and 1999-2003 legislatures. Both Christian-democratic parties were *always* part of the governing coalition except from 1999 onwards. Both socialist parties were *always* part of the governing coalition except during the 1974-77 and 1981-87 legislatures.

However, while the 1974-77 and 1999-2003 legislatures were characterised by rather loose fiscal policy, the 1981-87 legislatures were characterised by considerable fiscal consolidation efforts. This is another paradox given that according to our measure the liberal-conservatives were less certain of re-election[43]. Interestingly, the fiscal loosening up after 1999 implemented by a coalition including the liberal-conservatives consisted to an important extent of a tax decrease voted in 2001. Hence we should understand "a debt funded expenditure increase" to possibly take the shape of a *targeted tax decrease* not compensated by a tax increase elsewhere. Between 1999 and 2011 Didier Reynders was without interruption the Francophone liberal-conservative finance minister of Belgium. The opposition and the press suspected him during his entire ministership not to care as much about a

[42] Similarly, in his empirical paper on the effect of uncertainty of re-election on Swedish municipal debt, Pettersson-Lidbom (2001) operationalises "expectation of losing an upcoming election" by "relative frequency of having lost *previous* elections".

[43] A possible explanation of this paradox is that the 1981-1987 coalition governed with the support of only a *minority* of the Francophone Belgian parliamentary seats. Precisely these seats represented (centre-right) Francophone Belgian voters who may have believed that they *did* have a stake in fiscal consolidation, i.e. that they were among the voters who disproportionately paid the price of debt accumulation. These coalitions are the only ones over our sample period not disposing of a majority in both language groups in the federal parliament, which could mean that such governments are hard to assemble or maintain.

balanced budget as about targeted tax reductions for his –liberal-conservative- constituency[44]. (see e.g. Knack 2009 and De Tijd 2009) During his reign there were few years with a budget surplus – similarly to our entire period studied (1970-2002)-, and the primary surplus strongly decreased (not shown in table 8 below however, as our period studied ends in 2002).

Hence Persson and Tabellini (2000 p. 345-372)'s prediction that parties less certain of being in power run higher deficits seems only partly to apply to the track record of the Belgian liberal-conservative parties. It even seems to apply less to the Belgian socialist and Christian-democratic parties: while being highly certain of being in power, they have apparently not been able/willing to prevent high deficits either. This inability/unwillingness seems to have culminated in the 1979-1981 period, during which the primary deficit attained its maximum over our sample period, and which was governed by Christian-democratic – socialist coalitions almost without interruption. Perhaps even a reverse causality held: it might have been that precisely (their apparent commitment to) the running of large deficits assured Christian-democrats and socialists of *remaining in power*. This would be a considerable criticism to the theory of Persson and Tabellini.

2.2.2. The debt game *within the same government* applied to Belgium

The large Belgian federal public debt not only seems to be explained by political instability in terms of the changing party-political composition of successive Belgian Federal governments. An additional explanation seems to be the number of large, short-lived and uncohesive coalition governments –in the wording of Roubini and Sachs (1989). Most post-war central governments in Belgium were coalitions between Christian-democrats and socialists. Since the 1970s, a decade during which all 3 major Belgian parties split into a Flemish and a Francophone wing, this has implied government coalitions of at least 4 parties, of which the Flemish parties and the Francophone Belgian parties collected their votes in completely different electoral districts[45].

While over our period studied (1970-2002) there were 10 federal elections –i.e. on average 1 election per 3 years while a full legislature in Belgium lasts 4 years-, there were no less than 20 governments – i.e. on average 1 government per 1.5 year. Moreover, between 1970 and 1981 there were no less than 5 elections –on average one every 2.4 years– and no less than 12 governments –i.e. on average 1 government per year. The 1970-1981 subperiod corresponds to our 1st subperiod in table 8 below, marked by a mounting primary deficit, a mounting budget deficit, a mounting public debt, a mounting interest bill, and a primary deficit even for Flanders[46].

[44] Including of an indirect but substantial reduction in the corporate income tax for a number of large companies, and including of willingly overestimating tax revenues so as not having to increase taxes.

[45] With the exception of the electoral district of Brussels-Halle-Vilvoorde, where all parties represented in the federal parliament kept running for election.

[46] The fiscal worsening over the 1970-1981 of course cannot be considered in isolation from the economic crisis that started in the early 1970s. Probably political factors and economic factors interacted during that period, with a severe worsening of fiscal outcomes as a result. This reminds of the "difficult times" concept of Perotti and Kontopoulos (2002 p. 197).
However, remarkably there were only 2 elections and 2 government dissolutions that led to a change in coalition composition during the 1970-1981 period: in 1974 when the Christian-democrats exchanged the socialists for the liberals as coalition partners, and in 1977 when the reverse happened. The number of governments and elections during this period may indeed rather be a measure of political fragmentation than of political instability.

2.2.3. The Belgian exception: the debt game between *different regions* represented in *the same government*

With respect to the *simultaneous* partisan dimension to political economy motives for central government debt financing in Belgium, it is important to stress that Belgian political parties are linguistically and hence largely geographically split[47]. This split seems to narrow political parties' mandates -and to disperse their power over fiscal policy- even more than in other systems of coalition governments. In the wording of Persson and Tabellini (2000 p. 345): such a split increases the number of veto rights over fiscal policy as well as their probability of being used. As a matter of fact, such veto rights were already established in Belgium by means of the 1970 "state reform", i.e. decentralisation round:

- the legislative was divided into a "Dutch-speaking group" and a "French-speaking group"[48], and any 2/3 of members of parliament of any group are since then entitled to block any law from being approved;
- the executive –which in Belgium decides by consensus- since then needs to consist of an equal number of ministers belonging to either parliamentary group

It should be admitted that also in some other countries there is a strong correlation between geography and strength of a particular political party, such as in the UK (Tories over the last decades collecting mostly votes in the south, Labour over the last decades mostly collecting votes in the geographic centre and the north) and in India (perhaps resembling Belgium the most in this respect, as several parties represented in the Indian federal parliament only collect votes in one particular state). But what is typical of Belgium is that:

- *all* of its parties are regional and
- the constitution *guarantees* both main language communities *equal* representation within the Federal government

As far as we know, the additional potential consequences of such a "Belgian" setting for debt accumulation –i.e. the Belgian *simultaneous regional* dimension- have not been investigated in the existing literature on the political economy of budget deficits. An exception are Lago-Peñas e.a. (2014), who find empirical cross-country evidence for the negative impact of the regionalisation of party systems on fiscal performance: the lower the "nationalisation" of a party system, the larger the common pool problem and the lower the primary fiscal balance. Lago-Peñas e.a. (2014) thereby characterise Belgium as one of the few cases of extreme party system regionalisation in the world[49].

[47] As in Belgium geography and language overlap to a large extent. Flemish overwhelmingly live in the northern part of Belgium, Flanders, while Francophone Belgians overwhelmingly live in the southern part of Belgium, Wallonia, as well as in the Brussels region.

[48] In practice, members of the Dutch-speaking group are almost all members of parliament elected in Flanders, and most members of the French-speaking group are elected in Wallonia. The electoral district of Brussel-Halle-Vilvoorde, straddling the regions of Flanders and Brussels, is the only district "generating" French-speaking as well as Dutch-speaking members of parliament.

[49] In contrast, in their study of political-economy drivers of the budget deficit Roubini and Sachs (1989 p. 908) simply classify Belgium as another system of "multi-party coalitions with short tenure", in the same category as e.g. Denmark and the Netherlands. However, elsewhere (p. 930) they put forward the case of Belgium "with weak coalition governments with parties that are divided along linguistic and geographic lines as well as ideological lines" as a case study meriting further research.

Another exception may be the research by Huber e.a. (2003), who find power dispersion within coalitions as measured by the Banzhaf voting power index to be a determinant of the size of budget deficits. The Banzhaf power index measures the number of feasible coalitions with a view to obtaining a majority in which a particular party is needed, as a share of the total number of feasible coalitions. In Banzhaf's terminology, one could state that the power indices of the Flemish parties taken together and of the Francophone Belgian parties taken together are both 1, i.e. maximal. This is because –as said above- constitutionally ministers are to be proposed by Flemish and by Francophone Belgian parties in a 50-50% proportion, and because constitutionally both language groups have got de facto veto power in the federal parliament. Huber e.a. (2003) find that coalitions with equally strong partners –in terms of the Banzhaf power index- run significantly higher deficits than coalitions with 1 dominating party. Huber e.a. (2003 p. 342) hypothesise that this is because the absence of a dominant party makes possible a lasting "war of attrition"[50] or stalemate between the coalition partners –in terms of damaging expenditure increases, in turn increasing the deficit.

The uncertainty aspect that is important to the *partisan dimension over time* to political economy motives for debt financing is no longer present within the *simultaneous regional dimension* to political economy motives for debt financing typical of the Belgian setting. Indeed, *by law* a Belgian Federal government needs to consist of ministers proposed by Flemish and by Francophone Belgian parties in a 50-50% proportion (although about 60% of Belgian population is Flemish). Typically for Belgium, the *uncertainty* of regions to be represented in the Federal government is institutionally *eliminated and replaced* by regional *polarisation* within this same Federal government. This polarisation is rather high since the Flemish and Francophone Belgian parties' resp. electoral districts –i.e. Flanders and Francophone Belgium[51]- are rather heterogeneous in expenditure preferences, largely due to their heterogeneous perception of socio-economic and demographic needs. Over our sample period (see below), this polarisation is witnessed by the fact that several of the 12 governments that characterise our 1970-1981 subperiod were dissolved prematurely because of conflicts between Flemish and Francophone Belgian parties[52].

Similar to the reasoning of Alesina and Perotti (1994 p. 17) and Persson and Tabellini (2000 p. 354), geographic polarisation within the executive seems to provide an additional incentive to parties only collecting votes in Francophone Belgium, i.e. in Wallonia and Brussels, to make the central government run a deficit. This is because our below calculations show that debt-financing is more profitable / less costly in fiscal terms for Wallonia and Brussels than for Flanders[53].

[50] The war of attrition (see Rasmusen 2006 p. 76) is a timing game that is similar to but slightly different from the better known and earlier mentioned prisoner's dilemma.

[51] We consider the Brussels region as part of Francophone Belgium in our political economy analysis of the Belgian deficit. Over our period studied, the number of voters voting for Flemish parties in the Brussels Region has continually decreased. During the 2014 elections for the Brussels Parliament e.g., all Flemish parties together obtained merely 11.35% of the vote.

[52] Although several of these conflicts were conflicts about decentralising the central government, not fiscal conflicts in their narrow meaning.

[53] Therefore, a path of further research could be to test empirically for a panel of countries if large interregional fiscal and income disparities –possibly in combination with high political polarisation and fragmentation- lead to large deficits. Such a test could add important political-economy explanations of variation in deficits to the ones already found by Persson and Tabellini (2003 p. 179-183) in "The economic effects of constitutions", an empirical follow-up to their 2000 classic. A potential problem with such a panel analysis is that our independent variables of interest are not expected to vary considerably over time. This is illustrated for Belgium by the rather stable regional expenditure and revenue shares over time that are visible from columns 1 and 2 of table 8 below. Additionally, such independent variables may be eliminated from our analysis when including country fixed

In Persson and Tabellini (2000 p. 345)'s wording one could describe the Belgian common pool problem between the Flemish and the Francophone Belgian parties as a poor definition of Flanders' and Francophone Belgium's property rights over federal tax revenue. In Belgium, different geographic consequences of debt financing overlap to a large extent with different intra-coalition consequences of debt financing. While the existing political economy literature on deficits and debt seems to explain the overall size of deficits and debt purely by *non-geographic* political economy factors, an extra challenge for Belgium seems to be to explain the overall size of deficits and debt by the *geographically unevenly spread costs and benefits* stemming from deficits and debt.

Debt financing may be cheaper for Wallonia and Brussels not only in fiscal terms but also in broader economic terms: as the private sector is larger in Flanders than in Wallonia and Brussels combined –both in absolute and in relative terms-, a worsening of the macro-economy due to mounting debt may be predominantly felt in Flanders. Moreover economic spill-overs between Flanders and Wallonia seem limited in important respects[54].

Finally, the relatively lower cost of deficit financing for Wallonia provides it with an incentive not only to let the Federal government *accumulate debt* but also to *postpone fiscal consolidation*, i.e. to postpone reducing the deficit. Alesina and Drazen (1989) model consolidation postponement –i.e. postponement of *agreement on the burden sharing* of consolidation- as another war of attrition. In this war of attrition the party that bears most of the cost of postponement (in our case Flanders, while Wallonia has an incentive to postpone consolidation) eventually gives in, in that it also eventually accepts to bear most of the cost of consolidation. From columns 3 of table 8 (see below) it may be derived that after the coming to power of a turnaround central government coalition in 1981 –which governed during 2 successive legislatures until 1987- the primary balance of Flanders improved more strongly than the primary balance of Wallonia. The Flemish primary balance improved from its lowest point of -4.5% of Belgian GDP in 1980 to 3.2% in 1987 (i.e. with 7.7%points), while the resp. numbers for Wallonia were -5.7 and -1.4% (i.e. an improvement of 4.3%points).

However, an alternative reasoning could be that Wallonia and Brussels have an incentive to provoke a federal budget deficit exactly because they may anticipate to be largely *exempted* from fiscal consolidation. In this case Wallonia and Brussels would *not* have a particular incentive to postpone fiscal consolidation. This is because Wallonia and -since the 1990s- also Brussels are poorer than Flanders, due to which Francophone Belgian parties could anticipate that a tax increase would probably result in a relatively larger revenue increase originating in Flanders than originating in the rest of Belgium. This seems to hold in particular for the personal income tax, which is rather strongly progressive in Belgium, and for social security contributions, which are not paid by people living on an allowance. Moreover, over our sample period consolidation in Belgium seems to have had a higher chance of being *revenue* based than of being *expenditure* based. This may have been because both socialist parties have been part of all federal coalitions over our sample period, except between 1974 and 1977 and between 1981 and 1987[55]. I.a. due to socialist coalition presence social expenditures –

effects. The latter is appropriate in panel data analysis, while country fixed effects appear absent from the analysis of Persson and Tabellini (2003 p. 179-183).

[54] A rather visible example is commuting: there is little commuting between Flanders and Wallonia, the 2 major regions of Belgium. (see e.g. Persyn and Torfs 2015) There is even less migration between those regions. A counterexample may however be interregional importing and exporting.

[55] The 1981-87 consolidation effort seems to have been the only consolidation effort over our sample period that was predominantly expenditure based.

predominantly favouring Wallonia and Brussels- may have been largely exempted from consolidation efforts over our sample period[56].

3. Calculating interregional net fiscal transfers resulting from the Belgian federal public debt

In this section we first propose a methodology for calculating interregional net fiscal transfers resulting from central government debt, which we illustrate with a simple numerical example. Next we effectively calculate interregional transfers due to the interest bill on the Belgian federal public debt following our proposed methodology. We conclude with an attempt at a regional cost-benefit analysis of the Belgian federal public debt.

3.1. A methodology for calculating interregional net fiscal transfers resulting from central government debt

In this subsection we propose a methodology to calculate interregional net fiscal transfers resulting from the interest bill on the Belgian federal public debt[57] (put more briefly: net fiscal transfers resulting from the federal public debt, or even more briefly: interest transfers), and derive some policy implications from this calculation. We do not know of attempts other than on Belgium to investigate the consequences of public debt for interregional net fiscal transfers. Examples are Algoed (2009), Van Rompuy and Bilsen (1988), Van Gompel (2004), and Meunier e.a. (2006). Algoed (2009) proposes a methodology for calculating interregional net fiscal transfers resulting from the federal public debt. In the spirit of Algoed (2009), Van Rompuy and Bilsen (1988) and Van Gompel (2004) effectively calculate Belgian interregional net fiscal transfers resulting from the federal public debt, finding that interest transfers amount to 55% of total interregional net fiscal transfers in 1985, resp. to 37% of total interregional net fiscal transfers in 2003.

Key is that the 3 latter studies define net transfers from the central government debt in a particular year as the difference between the part of the interest bill of a particular region corresponding to the part of the central government debt this region has "*caused*", i.e. corresponding to the part of this region in the cumulative primary deficits of the past, *and* the part of the interest bill a particular region *effectively pays*.

Compared to these earlier calculations of interregional net transfers resulting from the federal public debt, the current attempt covers more years –because of which the current attempt finds net transfers amounts that are even larger than the amounts found by Van Rompuy and Bilsen (1988) and Van Gompel (2004). The current attempt also situates the result of our calculations within the theoretical literature on the political economy of debt financing (see above).

[56] A 3rd alternative reasoning is along the lines of Alesina and Drazen (1989 p. 2), who predict any anticipated polarised –i.e. uneven- geographic incidence of any future budget consolidation *itself* to postpone consolidation efforts.

[57] We hence disregard in our analysis public debts of the lower level governments of Belgium, which besides are negligible compared to the Belgian federal public debt.

As said already, in Belgium in most years the federal budget is not balanced but shows a deficit. In any country, any central government budget deficit is to some extent unevenly spread over the regions that compose that country[58]. I.e. in any country when disaggregating the overall central government budget geographically it will appear that some regions "run" a bigger central government primary deficit than others, or even that some regions "run" a central government primary surplus while others "run" a central government primary deficit. Regional disaggregation of an overall central government budget boils down to calculating what expenditures and revenues of each individual region would have been *if they would have been independent countries, however keeping policy constant*[59].

This year's –calculated, i.e. implicit- central government budget deficit of a particular region adds to the previous year's –calculated, i.e. implicit- central government public debt of that same region, and impacts next year's –calculated, i.e. implicit- central government interest bill caused by that region. Interest payments constitute an expenditure category which would not be part of the budget in the absence of previous budget deficits, and which hence –of course- would not cause interregional net fiscal transfers. Once the interest bill appears as a central government expenditure item, it is likely to result in interregional net fiscal transfers. I.e. interest payments are likely to result in some regions –effectively but implicitly- paying a larger part of it than on the basis of their share in past central government primary deficits, and in some regions –effectively but implicitly- paying a smaller part of it.

However, once budget surpluses are run and as a consequence the public debt starts to decrease, a final type of interregional net fiscal transfers emerges, i.e. interregional net fiscal transfers resulting from *paying down* the public debt. This is because:

- a budget surplus effectively pays down part of the public debt thanks to tax revenue that is larger than public expenditure and that has been "provided" to a different extent by the different regions of the country concerned
- however, the share of the public debt that a particular region *would* have paid down on the basis of its share in the central government public debt, i.e. on the basis of its share in past cumulative central government primary balances, may differ from the share of the central government public debt that a particular region has *effectively* paid down, i.e. considering its share in central government tax revenue

We will also consider this final type of net fiscal transfers in our numerical example for a fictitious country (see below). However, in our calculation of real interregional net fiscal transfers resulting from the federal public debt in Belgium (see section 3.3), by far the largest part of them will consist of transfers caused by the interest bill. This is mainly because over our period considered (1970-2002) -and also thereafter-, budget surpluses have been rare and small (see column 4 of table 8 below).

As a result of the above reasoning we arrive at the following definition of interregional interest transfers:

[58] The same holds for any budget surplus.

[59] In Belgium, as in most other countries, the central/federal government does not make regionally disaggregated data on federal taxing and spending in their entirety available, in particular with respect to taxation, and with respect to spending that runs *from the government to the citizen*, instead of with respect to spending in the form of *intergovernmental grants*. In Belgium, data on grants from the Federal government to the "states", called Regions and Communities in Belgium, are readily available at http://financien.belgium.be/nl/binaries/B7._nl_tcm306-254445.pdf , but they constitute only a part of total Federal government spending, and they have become sizeable only from 1989 onwards.

Interregional transfers resulting from the interest bill on the central government debt

EQUAL

the part of the interest bill to pay by that particular region that corresponds to its part in the central government debt

MINUS

the part of the interest bill that a particular region *effectively* pays (i.e. corresponding to that particular region's part in central government tax revenues) [60].

Hence interregional interest transfers stem from the difference between:

- on the one hand the part of the central government expenditures financed by borrowing that a particular region *would* have benefited from if it corresponded to the share of the interest bill this region bears through its contribution to central government tax revenues
- and on the other hand the part of central government public expenditures financed by borrowing that has *effectively* benefited a particular region

In what follows we will first apply the above definition to a fictitious numerical example, after which we will apply the hence explained methodology of interest transfers calculation to the case of the Belgian regions. We end with a number of conclusions.

3.2. A simple numerical example for a country with only 2 regions

Before proposing a calculation of interregional interest transfers in Belgium, we explain our methodology by means of a fictitious numerical example[61], which will prove to be much more simple than the Belgian situation, but still to share important characteristics with the Belgian case (as will become clear in section 3.3).

Table 2 describes the starting year T1 in a country with 2 regions A and B (and one central government), which run a different -central government- primary balance (row 4) because they contribute the same amount to –central government- tax revenues (row 2) but benefit to a different extent from –central government- public expenditures (row 3)[62]. However, both regions run a –central government- primary deficit. (This is a situation resembling the Belgian situation until the early 1980s. See section 3.3 below.) We assume for the sake of simplicity that all interests to be paid on the debt that is caused by the primary deficit run in year T1, start becoming due only in year T2[63]. Therefore interests due in year T1 are zero (rows 5 and 6), and hence also net fiscal transfers caused by the interest bill (rows 7 and 8). Therefore at the end of year T1 the primary balance equals the budget balance (row 9).

[60] This is also the criterion used and explained in Algoed (2009).
[61] Note that in what follows sums do not always add up exactly, due to rounding. We have decided to show only 1 decimal number in tables 2 to 7.
[62] It is the differing shares in the primary deficit that are essential to our reasoning. If these differing shares –as opposed to table 2- would be due to differing shares in tax revenue collected, rather than to differing shares in (primary) spending, or to both, our reasoning would fundamentally stay the same.
[63] This assumption does not impact the size of the interregional interest transfers over our entire period considered.

Table 2: T1: Both regions run a primary deficit, but of a different size

T1		Region A	Region B	Whole country
1	Debt (at **beginning** of period)	0	0	0
2	Tax revenue collected	45	45	90
3	(Primary) spending received	55	60	115
4 = 2-3	Primary balance	-10	-15	-25
5	Interests actually paid	0	0	0
6	Interests to pay given share in debt	0	0	0
7	**Transfers due to interest burden**	**0**	**0**	**0**
8	PM: Interest transfers as share of interest burden	0%	0%	0%
9 = 4-6	PM: Budget balance	-10	-15	-25
10 = e.g. for Region A (45/90)*115	PM: Regional distribution of tax revenue assuming all spending would be tax funded	57.5	57.5	115
11 = 3-10	**PM: Transfers calculated the traditional way**	**-2.5**	**2.5**	**0**
12 = (11 as a share of last column of 3)	PM: Transfers calculated the traditional way as share of total (primary) spending	-2%	2%	0%

However, because of the different regional shares in spending compared to tax revenue (rows 2 and 3), interregional fiscal transfers in year T1 are *not* zero when we calculate them the way they have traditionally been calculated for Belgium as well as for other countries[64], as shown in row 11. In row 11 tax revenue is increased proportionally for each region until total tax revenue equals total spending, under the –simplifying but unrealistic- assumption of a balanced budget. As a next step interregional net transfers are obtained in row 11 by subtracting row 10 from row 3[65].

Now suppose that in year T2, regional primary balances stay the same as in year T1, a situation shown in table 3. Hence the difference with year T1 is that now interest charges have to be paid, i.e. on the debt incurred in year T1 of our numerical example[66]. For the sake of simplicity, we assume that the interest rate remains constant at 10% in every year. We also assume that the entire interest bill is paid with tax revenue, as the interest bill is evidently a priority public expenditure. The –plausible- assumption of payment of the interest bill with tax revenue has consequences for the calculation of interest transfers: as both regions again contribute the same amount to total tax revenue in year T2, they *de facto* pay an equal part of the interest bill (row 5). However, as the share of region B in the public debt is the higher one (row 1), region B *would* pay a *larger* share of the interest bill than region A if this share corresponded to region B's share in the public debt (row 6)[67]. This difference leads to interregional interest transfers (row 7).

Although we assume that the interest bill –as a priority expenditure- is paid with tax revenue, we assume that interest expenditure does not crowd out existing primary expenditures, but simply adds to it, i.e. the central government allows total expenditure to increase with the interest bill[68]. As a

[64] For an example for Belgium see Algoed (2008). For examples for other countries see Dubay (2006) for the US and Bird and Vaillancourt (2007) for Canada.
[65] We present outgoing transfers as a negative amount and incoming transfers as a positive amount.
[66] Of course another obvious difference is that the public debt at the start of year T2 is no longer zero.
[67] I.e. if region B would be an independent country but with the same fiscal policy.
[68] This accommodating stance implies that the central government allows the interest bill in turn to worsen the fiscal situation, opening the door to a so called interest snowball: deficits not only increasing the interest bill, but also the interest bill increasing the deficit. Such a snowballing effect risks in particular to occur if the interest rate is higher than economic growth and it can even occur in a situation of a primary surplus. Belgium effectively suffered from an interest snowball between 1977 and 1988, between 1991 and 1993, and from 2009 onwards. (NBB 2010 p. 168)

consequence the budget deficit (row 9) increases with the interest bill (row 6) compared to year T1. As another consequence public debt at the beginning of year T3 (row 1 in table 4) of course not only increases with the primary deficit (i.e. with the opposite of row 4) but also with the interest bill (row 6).

Table 3: T2: Both regions run a primary deficit, but of a different size (bis)

T2		**Region A**	**Region B**	**Whole country**
1 = (1 in T1 - 4 in T1 + 6 in T1)	Debt (at **beginning** of period)	10	15	25
2	Tax revenue collected	45	45	90
3	(Primary) spending received	55	60	115
4 = 2-3	Primary balance	-10	-15	-25
5 = e.g. for Region A (45/95)*2.5	Interests actually paid	1.3	1.3	2.5
6 = (10% of 1)	Interests to pay given share in debt	1	1.5	2.5
7 = 6-5	**Transfers due to interest bill**	**-0.3**	**0.3**	**0**
8 = (7 as a share of last column of 5)	Interest transfers as share of interest bill	-10%	10%	0%
9 = 4-6	Budget balance (including interests due given share in debt)	-11	-16.5	-27.5
10 = e.g. for Region A (45/90)*115	PM: Regional distribution of tax revenue assuming all spending would be tax funded	57.5	57.5	115
11 = 3-10	**PM: Transfers calculated the traditional way**	**-2.5**	**2.5**	**0**
12 = (11 as a share of last column of 3)	PM: Transfers calculated the traditional way as share of total (primary) spending	-2%	2%	0%

Hence only from T3 onwards a region's share in public debt is composed of all of its 3 possible components, as expressed in the below formula:

$$D_i^t = D_i^{t-1} - PB_i^{t-1} + I_i^{t-1}$$

I.e. a region's share of the public debt at the beginning of period t equals:

- that region's debt share at the beginning of period t-1,
- minus that region's share in the primary balance run during period t-1,
- plus that region's share in the interest bill to pay in period t-1 *given its debt share*

The 3rd term, i.e. a region's share in the interest bill to pay in period t-1 *given its debt share*, equals the interest rate times that region's share in the public debt at the beginning of period t-1, and is expressed in the below formula:

$$I_i^{t-1} = i * D_i^{t-1}$$

Additionally, as a side-note, rows 10 to 12 allow us to further understand the relationship between interest transfers and net fiscal transfers as they are traditionally calculated, i.e. while implicitly assuming that all expenditures are tax funded. Because in T2 the amounts of tax revenue collected in regions A and B as well as the amounts of (primary) spending they received are the same as in T1, transfers calculated the traditional way are the same in T1 and in T2.

However, of course it would be more correct to explicitly take into account that –in Belgium as well as in our fictitious numerical example- (at least in most years) part of expenditure is debt financed. To show the consequences of this -more realistic but complicating- assumption is the aim of this section, with the cost of making net transfers calculations somewhat more complicated than previous

calculations of interregional net fiscal transfers in Belgium. While a traditional calculation of net fiscal transfers would have resulted in an amount of -2.5 for Region A in year T2, a more correct calculation results in only -0.3. I.e. at least in T2 interest transfers appear to be lower than net fiscal transfers according to their traditional calculation, which therefore are an overestimation of the true net fiscal transfers. One could say that the budget deficit of period T2 allows net fiscal transfers to be postponed into the future.

Suppose now that in year T3 the previous situation changes. Suppose e.g. that a newly elected central government implements fiscal consolidation measures in the form of tax increases and/or expenditure cuts that only –or mainly- affect region A (e.g. because of region A's economic characteristics being different from region B's, or because region A being less represented in the newly elected government). (This is a situation resembling –at least in terms of fiscal outcomes- the Belgian situation from 1983 until 1985. See section 3.3 below.) This situation is shown in table 4: tax revenue contributed by region A strongly increases (row 2) (while public expenditure benefited from by region A stays constant (row 3)), resulting in a primary surplus rather than a primary deficit generated by region A (row 4)[69].

Table 4: T3: One region runs a primary surplus, the other region runs a primary deficit

T3		**Region A**	**Region B**	**Whole country**
1 = (1 in T2 - 4 in T2 + 6 in T2)	Debt (at **beginning** of period) (including interests due given share in debt in t-1)	21	31.5	52.5
2	Tax revenue collected	60	45	105
3	(Primary) spending received	55	60	115
4 = 2-3	Primary balance	5	-15	-10
5 = e.g. for Region A (60/105)*5.3	Interests actually paid	3.0	2.3	5.3
6 = (10% of 1)	Interests to pay given share in debt	2.1	3.2	5.3
7 = 6-5	**Transfers due to interest bill**	**-0.9**	**0.9**	**0**
8 = (7 as a share of last column of 5)	Interest transfers as share of interest bill	-17%	17%	0%
9 = 4-6	Budget balance (including interests due given share in debt)	2.9	-18.2	-15.3
10 = e.g. for Region A (45/90)*115	PM: Regional distribution of tax revenue assuming all spending would be tax funded	65.7	49.3	115.0
11 = 3-10	**PM: Transfers calculated the traditional way**	**-10.7**	**10.7**	**0**
12 = (11 as a share of last column of 3)	PM: Transfers calculated the traditional way as share of total (primary) spending	-9%	9%	0%

Unsurprisingly, such a regional asymmetry with respect to tax revenue and expenditures increases net transfers calculated the traditional way considerably (row 11). However, the primary surplus of region A is not large enough to generate a country-wide primary surplus (last column of row 4). Because of the larger share of region A in tax revenue, the part of the interest bill actually paid by region A increases (row 5). In combination with the larger interest bill in general (compare last columns of rows 5 in tables 3 and 4), as well as with the increased part of the interest bill region B would pay if the part of the interest bill paid corresponded to the share in the public debt (compare rows 6 in tables 3 and 4), this leads to larger interest transfers as well (row 7).

[69] It is the primary surplus that is essential for our reasoning, not the way it is achieved (either by tax increases or expenditure cuts).
As an aside, it should be noted that we could also have opted for demonstrating the calculation of interregional interest transfers by taking regionally differing *budget balances* –rather than regionally differing *primary balances*- as our starting point. Our demonstration is however more clear when taking regionally differing primary balances as our starting point.

Once again a traditional calculation of net fiscal transfers overestimates the true net fiscal transfers. It results in an amount of no less than -10.7 for Region A, while a more correct calculation results in only -0.9.

Now suppose that –for the sake of the argument- the situation described in table 4 continues for another year, as shown by table 5. As a result of the primary surplus of region A in year T3, the debt to be assigned to region A (compare rows 1 of tables 5 and 6) is lower at the start of T4 than at the start of T3. Overall however, debt has still increased. These 2 facts combined increase interest transfers (row 7) further. Also notice that the share of interest transfers in the total interest bill (row 8) has only kept on increasing until now.

Because in T4 the amounts of tax revenue collected in regions A and B as well as the amounts of (primary) spending they received are the same as in T3, transfers calculated the traditional way are the same in T3 and in T4.

Table 5: T4: One region runs a primary surplus, the other region runs a primary deficit (bis)

T4		Region A	Region B	Whole country
1 = (1 in T3 - 4 in T3 + 6 in T3)	Debt (at **beginning** of period) (including interests due given share in debt in t-1)	18.1	49.7	67.8
2	Tax revenue collected	60	45	105
3	(Primary) spending received	55	60	115
4 = 2-3	Primary balance	5	-15	-10
5 = e.g. for Region A (60/105)*6.8	Interests actually paid	3.9	2.9	6.8
6 = (10% of 1)	Interests to pay given share in debt	1.8	5.0	6.8
7 = 6-5	**Transfers due to interest bill**	**-2.1**	**2.1**	**0**
8 = (7 as a share of last column of 5)	Interest transfers as share of interest bill	-30%	30%	0%
9 = 4-6	Budget balance (including interests due given share in debt)	3.2	-20.0	-16.8
10 = e.g. for Region A (45/90)*115	PM: Regional distribution of tax revenue assuming all spending would be tax funded	65.7	49.3	115
11 = 3-10	**PM: Transfers calculated the traditional way**	**-10.7**	**10.7**	**0**
12 = (11 as a share of last column of 3)	PM: Transfers calculated the traditional way as share of total (primary) spending	-9%	9%	0%

Next, table 6 describes year T5, in which both the primary surplus of region A and the primary deficit of region B improve sufficiently to achieve an *overall* primary surplus, but without region B achieving a primary surplus. (This is a situation resembling the Belgian situation from 1986 onwards. See section 3.3 below.) Because both regions now contribute a less unequal amount of tax revenue than in previous years (row 2), the new increase in interest transfers is entirely due to the increase in the debt of region B due to events in the previous year. Table 6 shows that interest transfers can be large and can even increase in years of an *overall primary surplus*. Notice however that in year T5 there still is an *overall budget deficit*.

Of course, because of the narrowed difference in regional primary balances, net transfers calculated the traditional way (row 11) *de*crease considerably between years T4 and T5. In contrast, interest transfers keep on increasing in spite of the improved overall fiscal situation.

Table 6: T5: The primary surplus of the surplus region improves, and also the primary deficit of the deficit region improves

T5		Region A	Region B	Whole country
1 = (1 in T4 - 4 in T4 + 6 in T4)	Debt (at **beginning** of period) (including interests due given share in debt in t-1)	14.9	69.6	84.5
2	Tax revenue collected	65	55	120
3	(Primary) spending received	55	60	115
4 = 2-3	Primary balance	10	-5	5
5 = e.g. for Region A (65/120)*8.5	Interests actually paid	4.6	3.9	8.5
6 = (10% of 1)	Interests to pay given share in debt	1.5	7.0	8.5
7 = 6-5	**Transfers due to interest bill**	**-3.1**	**3.1**	**0**
8 = (7 as a share of last column of 5)	Interest transfers as share of interest bill	-37%	37%	0%
9 = 4-6	Budget balance (including interests due given share in debt)	8.5	-12.0	-3.5
10 = e.g. for Region A (65/120)*115	PM: Regional distribution of tax revenue assuming all spending would be tax funded	62.3	52.7	115
11 = 3-10	**PM: Transfers calculated the traditional way**	**-7.3**	**7.3**	**0**
12 = (11 as a share of last column of 3)	PM: Transfers calculated the traditional way as share of total (primary) spending	-6%	6%	0%

Finally, table 7 describes year T6, in which also the primary deficit of region B turns into a primary surplus, which remains however smaller than the surplus of region A. (This is a situation resembling the Belgian situation from 1993 onwards. See section 3.3 below.) Remarkably, interest transfers keep increasing (row 7), and now even overtake net transfers calculated the traditional way (row 11).

An additional feature we added to year T6 is that it is not only the first year during which region B achieved a primary surplus (row 4), but also the first year during which an *overall budget surplus* (row 9) is achieved. (This is a situation resembling the Belgian situation in a limited number of years from 1995 onwards. See section 3.3 below.) Because of this budget surplus, in year T6 a de facto start is made with paying off the debt. Total debt decreases from 88 at the beginning of year T6 (row 1) to 81.8 (= 88-6.2) at the beginning of year T7 (not shown). As said above, as soon as a start is made with paying down the debt, a *new* kind of net transfers emerges (while net transfers due to the interest bill possibly keep increasing -due to events in the previous year- as effectively shown by the evolution from row 7 in table 6 to row 7 in table 7). This new kind of net transfers -the net transfers due to paying down the debt- is shown in row 12. It is calculated similarly to the net transfers due to the interest bill, i.e. as the difference between the part that a region *would* take in paying down the debt on the basis of that region's share in total debt (row 11) and the part that a region *actually* takes in paying down the debt –i.e. simply its part in the overall budget balance minus its part in interests actually paid in year T6 (row 10)[70].

So paradoxically, once a start is made with paying down the debt, net transfers resulting from the public debt increase rather than decrease (in our example with 5.1 in year T6, plus with 0.6 due to increased interest transfers that are in turn due to events in the previous year). The increase with 5.1 is due to the fact that region A actually pays down a larger part of the debt than the part it would pay down on the basis of its share in the total debt[71].

[70] Also this kind of transfer can be considered a postponement of traditionally calculated fiscal transfers.

[71] Because table 8 (see below) shows that Belgian federal budget surpluses have been rare and small over our entire period considered, in table 8 we do not separately calculate the additional transfers caused by the debt paid down thanks to those budget surpluses.

Of course, because of the once again narrowed difference in regional primary balances, net transfers calculated the traditional way (row 11) again *de*crease considerably between years T5 and T6. In contrast, interest transfers keep on increasing in spite of the improved overall fiscal situation.

To sum up, common threads running from T1 to T6 are the following:

- the constantly worse fiscal situation of region B –in combination with continuing overall budget deficits and hence an ever increasing overall debt burden- leads from T2 onwards both to net transfers resulting from the interest bill and to net transfers calculated the traditional way,
- but the former transfers show a snowballing behaviour, steadily increasing year after year, and even increasing when the overall fiscal situation starts to improve, while the latter do not at all show such an effect; this snowballing behaviour is essentially due to a situation of non-decreasing debt, composed of past primary deficits in which the 2 regions have unequal shares, and on which each year interests have to be paid
- the constantly worse fiscal situation of region B –in combination with continuing overall budget deficits and hence an ever increasing overall debt burden- leads after a while to a situation in which net transfers calculated the traditional way are an underestimation of real net transfers, i.e. including net transfers resulting from the interest bill
- there does not seem to be an upper limit to interest transfers: if debt keeps increasing, and shares in the primary deficit remain unequal, interest transfers will keep increasing as well (in our calculation of interest transfers for Belgium over the 1970-2002 period, we will show that interest transfers can become even larger than the total interest bill; see section 3.3 below)

Table 7: T6: *Both* regions run a primary surplus, but of a different size, and an *overall* budget surplus is achieved

T6		**Region A**	**Region B**	**Whole country**
1 = (1 in T5 - 4 in T5 + 6 in T5)	Debt (at **beginning** of period) (including interests due given share in debt in t-1)	6.4	81.6	88.0
2	Tax revenue collected	65	65	130
3	(Primary) spending received	55	60	115
4 = 2-3	Primary balance	10	5	15
5 = e.g. for Region A (65/130)*8.8	Interests actually paid	4.4	4.4	8.8
6 = (10% of 1)	Interests to pay given share in debt	0.6	8.2	8.8
7 = 6-5	**Transfers due to interest bill**	**-3.8**	**3.8**	**0**
8 = (7 as a share of last column of 5)	Interest transfers as share of interest bill	-43%	43%	0%
9 = 4-6	Budget balance (including interests due given share in debt)	9.4	-3.2	6.2
10 = 4-5	Debt actually paid down	5.6	0.6	6.2
11 = e.g. for Region A (6.4/88)*6.2	Debt to pay down given share in debt	0.5	5.7	6.2
12 = 11-10	**Transfers due to paying down the debt**	**-5.1**	**5.1**	**0**
13 = 7+12	**Total transfers due to the public debt**	**-8.9**	**8.9**	**0**
14 = e.g. for Region A (65/130)*115	PM: Regional distribution of tax revenue assuming all spending would be tax funded	57.5	57.5	115
15 = 3-14	**PM: Transfers calculated the traditional way**	**-2.5**	**2.5**	**0**
16 = (15 as a share of last column of 3)	PM: Transfers calculated the traditional way as share of total (primary) spending	-2%	2%	0%

3.3. Calculating interregional net transfers due to the interest bill on the Belgian federal public debt

Because we only have the necessary data from 1970 onwards, we can only calculate interest transfers from 1970 onwards. We only consider the federal budget –including social security-, as only the federal budget is able to generate net fiscal transfers between the 3 Belgian regions. Our data source for regional shares in federal expenditures and revenues is Van Gompel (2004), which is in turn based on Van Rompuy and Bilsen (1988) and Dottermans (1997). These sources have calculated the regional distribution of all federal revenues and expenditures through *approximation* of these distributions by:

- relevant regional population shares (e.g. share of the young with respect to regional disaggregation of school grants and child benefits, share of over 60 years old with respect to regional disaggregation of retirement benefits)
- gross regional product (corrected for gross incomes of commuters to other regions so as to avoid an overestimation of the share of Brussels, the Belgian region with by far the largest share of employees living in another region) (gross regional product is used for e.g. regional disaggregation of government purchases of goods and services and for regional disaggregation of VAT)
- gross regional value added (for the corporate income tax)
- number of employees weighted by gross wage bill and number of self-employed weighted by their value added (both for regional disaggregation of social security contributions).

It should be noted that the Federal government does not publish regionally disaggregated data on federal expenditures and revenues. Recent exceptions are data on the region of residence of federal civil servants (allowing to distribute the federal public wage bill over the Belgian regions), regional shares in the personal income tax, regional shares in the number of unemployed, regional shares in health spending and –published since Belgium became fiscally speaking to a considerable extent a decentralised country in 1989- regional shares in the federal grants to the Belgian Regions and Communities. Another exception are the so called "Regional accounts" of the NBB, which disaggregate geographically *since 1995 only* income tax revenue, social security contributions, and social expenditures, and which would enable us to approximate regional primary balances with regional differences between social security contributions and social expenditures. However, we prefer to use the data of Van Gompel because Van Gompel provides a regional disaggregation of federal revenues and expenditures *as a whole* for every single year *between 1970 and 2002*, a period crucially encompassing the years of build-up of the large Belgian federal debt.

We could have extended our sample period until 2010, as CERPE (2011) calculates regional primary balances for the 2006-2010 period[72], but because of possibly different methodologies for the calculation of these primary balances, we have chosen not to. CERPE (2011) obtains smaller primary surpluses for Flanders over its period considered compared to the last years of our period, while obtaining larger primary deficits for Wallonia. Extension of our sample period until 2010 would only have made the result of our calculations more dramatic (see below).

[72] Likewise, Jennes (2014a) calculates interregional transfers the traditional way for the 2007-2011 period.

Assumptions made for our net transfers calculation:

1) For the starting year 1970 we assume –due to lack of data for preceding years- that regional shares in the public debt equal regional shares in tax revenue, i.e. we assume that the more a region contributed to tax revenue, the more it benefited from spending financed by borrowing, thereby avoiding interest transfers[73]. This is a conservative/prudent assumption, as Dottermans (1997) finds transfers from Flanders to Wallonia –calculated the traditional way- already from 1964 onwards. Combined with the fact that the build-up of the Belgian public debt already started before 1970 (though rather slowly compared to the years after 1970), these pre-1970 net transfers calculated the traditional way imply a share of Flanders in the public debt that is *lower* than its share in tax revenue.
   However, as an extra robustness check, we have even *more* prudently assumed that in 1970 the share of Flanders in the public debt was 100%. While this assumption of course reduces the interest transfers provided by Flanders since 1970, they remain very large, e.g. when compared to interregional net fiscal transfers in Belgium according to previous traditional calculations. E.g. while interest transfers out of Flanders amount to 5.6% of GDP (i.e. 14.6 billion euros) in 2002 –equalling 92% of the interest bill- when assuming that in 1970 regional shares in the public debt equal regional shares in tax revenue, the resp. numbers are 3.6% of GDP (i.e. 9.4 billion nominal euros) and 59% of the interest bill when assuming that in 1970 the share of Flanders in the public debt was 100%. As a final robustness check, we have assumed that in 1970 total Belgian public debt was zero, i.e. that the build-up of the Belgian public debt only started in the first year for which we have data available on regional primary balances. Under this assumption the interest transfers out of Flanders are hardly lower than under our baseline assumption (regional shares in the public debt equalling regional shares in tax revenue): 5.1% of GDP (i.e. 13.3 billion euros) in 2002 –equalling 84% of the interest bill.

2) We assume that the Belgian regions Flanders, Wallonia and Brussels were already relevant political actors in 1970. Indeed, it makes less sense to assign tax revenues and expenditures to geographic areas if those geographic areas do not simultaneously correspond to political institutions with responsibilities that potentially impact those revenues and expenditures[74].
   We see 2 arguments to treat our 3 regions already as key political actors from 1970 onwards.
   Firstly, the 2 of the 3 major political parties in the history of Belgium (the Christian-democrats and the liberals) split up into a Flemish and a Francophone Belgian political party,

[73] Assuming regional shares in tax revenue before 1970 were the same as in 1970.

[74] Some object to calculating net fiscal transfers between Belgian regions with the argument that e.g. Belgian provinces would be an equally relevant geographic division (or equally irrelevant, depending on one's view on calculating geographic net fiscal transfers). However, as opposed to Belgian regions, Belgian provinces hardly have competencies, let alone economic competencies. Hence, the possibly large net fiscal transfers between Belgian provinces are not able to have large consequences in terms of changed political behaviour, nor to be their result.
The above does however not imply that from a political economy perspective it would only make sense to calculate net fiscal transfers between politically relevant *geographic* entities. E.g. Decoster and Verwerft (2009) calculate net fiscal transfers (the traditional way) *between social groups that can be argued to be politically organised*, albeit possibly in a less clearcut way. I.e. Decoster and Verwerft (2009) calculate net fiscal transfers between income quintiles, between employed and unemployed, between home owners and tenants, and between the young and the old. That is, they calculate net fiscal transfers *across* Belgian regions and they find that transfers between the abovementioned social groups are much larger than transfers *between* Belgian regions. Therefore also *interest transfers* between those non-geographic entities are probably (even) much larger than interest transfers between Belgian regions.

resp. in 1971 and 1972. The 3rd major party, the socialist party, suffered from internal conflicts between Flemish and Francophone Belgian members throughout the 1970s, before also splitting up in 1978. This implied that from 1978s onwards, no single party represented in the federal parliament obtained votes nor even ran for elections both in Flanders and Wallonia[75]. Hence, we may broadly assume that from the start of our period, all Belgian political parties had an incentive to skew taxation and expenditure in favour of their region, exception made for the then still largely bilingual capital of Brussels.

Secondly, in 1970 a change of the constitution took place which had 2 major consequences:

- confederal organisation of the Federal government (as already explained above):
    - the legislative was divided into a "Dutch-speaking group" and a "French-speaking group", and any 2/3 of members of parliament of any group are since then entitled to block any law from being approved;
    - the executive –which decides by consensus- since then needs to consist of an equal number of ministers belonging to the either of both parliamentary groups
- start of the decentralisation process of the central government, i.e. start of process that would result in Belgium becoming a federal country: the 2 largest of the current 3 Belgian regions (Flanders and Wallonia) were established, while Brussels obtained special status as the capital of Belgium; also the current 3 Belgian communities were established (the Flemish, the Francophone and the German speaking communities)

But in 1970 only the communities acquired responsibilities, moreover only with respect to culture, due to which the Belgian central government kept control over the bulk of revenue and expenditure[76]. The 3 regions only obtained substantial expenditure responsibilities at the occasion of the 1980 constitutional change, including economic competencies.

Therefore, as a robustness check, we have only started calculating interest transfers in 1980, assigning the public debt at the end of 1979 to the regions according to regional shares in tax revenue. This change of starting date for our calculations reduces the interest transfers provided by Flanders at the end of our period, but they remain very large, e.g. when compared to interregional net fiscal transfers in Belgium according to previous traditional calculations. E.g. while interest transfers out of Flanders amount to 5.6% of GDP (equalling 14.6 billion euros) in 2002 –equalling 92% of the total interest bill- when starting to assign the public debt according to regional shares in tax revenue in 1970, the resp. numbers are 4.4% of GDP (equalling 11.5 billion nominal euros) and 72% when starting to assign the public debt according to regional shares in tax revenue in 1980.

3) As we only have data on regional shares in *total* tax revenue at our availability, we need to make the –minor- error to assume that the interest bill is paid out of *total* tax revenues, i.e. including social security contributions. In fact, as social security contributions are earmarked to social security expenditures, the interest bill is paid out of tax revenues *minus* social security contributions. We may however assume that this additional assumption does not have a major impact on our results, as over the 1970-2002 period regional shares in social security

[75] With the exception of the electoral district of Brussels-Halle-Vilvoorde, where all parties represented in the federal parliament kept running for election (as said above). We know only of India as another democracy and federal country in which a number of federal government coalitions have consisted to an important extent of parties that run for election only in one or a limited number of states. (see also Lago-Peñas e.a. 2014) However, in India the limited geographic and cultural basis of political parties supporting the federal government does not hold for the largest parties, and also is a more recent phenomenon, having come into existence only in the 1990s.

[76] With the exception of municipal taxation and expenditure.

contributions (residence basis) probably do not differ sufficiently from regional shares in tax revenues other than social security contributions, to considerably change our results. E.g. Denkgroep "In de Warande" (2005 p. 143 and 147) -one of the few existing calculations of interregional net fiscal transfers in Belgium over a considerable time period instead of over one single year- finds the share of Flanders in total federal tax revenues without social security contributions to be rather similar to the share of Flanders in social security contributions considered in isolation over the 1990-2002 period[77].

Table 8 summarises the method and the results of our calculations. As a first step, we have to calculate regional primary balances (expressed as a % of Belgian GDP) (columns 3), i.e. the differences between regional contributions to tax revenue (columns 1) and regionalised primary spending (columns 2) (after expressing columns 1 and 2 as a % of Belgian GDP). We notice that over our sample period, Flanders increases its share in tax revenue (columns 1) much more strongly than that it increases its share in primary spending (columns 2). In contrast, the (negative) gap between Wallonia's share in tax revenue and its share in primary spending remains about constant over our sample period. Still different is Brussels, which evolves from a share in tax revenue that is larger than its share in primary spending to the inverse situation.

These contrasting evolutions of columns 1 and 2 (expressed as shares of totals) result in *even more* contrasting primary balances (expressed as shares of GDP) (columns 3), precisely because total primary spending was considerably *larger* than total tax revenue during several years of our period considered. E.g. while the Flemish *share* in tax revenue was higher than its *share* in primary expenditures in any single year of our sample period, Flanders still ran a primary deficit in every single year before 1983. Conversely, while the Walloon *share* in tax revenue was lower than its *share* in primary expenditures in any single year of our sample period, Wallonia still ran a primary surplus from 1993 until and including 1997. This was because total primary spending was considerably *smaller* than total tax revenue during that latter period.

The Flemish primary balance is negative during the 1st subperiod that we are able to distinguish, 1970-1982[78], but turns positive during the next 2 subperiods we are able to distinguish, 1983-1994[79] and 1995-2002[80]. From 1986 onwards the Flemish surpluses are large enough to make the overall primary balance turn positive. But in particular the 3rd subperiod –which we distinguish because from 1995 onwards[81] also the overall *budget* balance (column 4) turns slightly positive in several years- is marked by very large Flemish primary surpluses. In contrast, except for the years 1993-1997 the Walloon primary balance remains negative over our entire period (and becomes very negative towards the end of the 1st subperiod). Brussels evolves broadly from primary surpluses in the 1st subperiod, over primary deficits in the 2nd subperiod, back to primary surpluses in the 3rd subperiod.

[77] Moreover, as we will find out in table 8 below, interregional net interest transfers in Belgium are to a far larger extent explained by diverging regional shares in the interest bill *caused* than by diverging regional shares in the interest bill *paid*.

[78] This 1st subperiod broadly corresponds to years T1 and T2 of our numerical example (tables 2 and 3).

[79] The 1983-1994 subperiod was preceded by the December 1981 elections, which resulted in a coalition change, resulting in turn in a prolonged fiscal consolidation effort until 1987. This 2nd subperiod broadly corresponds to years T3 and T4 of our numerical example (tables 4 and 5).

[80] This 3rd subperiod broadly corresponds to years T5 and T6 of our numerical example (tables 6 and 7).

[81] In 1992 a new government took office, which was the start of a prolonged budget consolidation until 1999 with a view to acceding to the euro zone. The 1992 government was re-elected in 1995. In contrast to the 1981-1987 coalitions, the 1992-1999 coalitions *did* have a majority of Francophone Belgian parliamentary seats at their disposal. Also in contrast to the 1981-1987 governments, the fiscal consolidation efforts of the 1992-1999 governments relied somewhat more on tax increases than on expenditure cuts.

As a next step, we calculate regional –"caused"- shares in the total public debt (at the beginning of each year) (expressed as a % of Belgian GDP) (columns 6)[82]. Crucially, in each year, a region's debt share is calculated as the sum of its debt share at the beginning of the previous year, its primary deficit run over the previous year (or the opposite of its primary surplus in case of a primary surplus), and its –"caused"- share in the interests that are due in the previous year. The latter simply corresponds to its debt share in the previous period (expressed in % of total debt) multiplied by the total interest bill (columns 7)[83]. Hence, when having calculated –"caused"- regional debt shares for a particular year, we can easily calculate –"caused"- regional interest bill shares for that same year. In other words the regional "caused" debt shares are the debts the resp. regions would have accumulated under the assumption that the 3 regions would have been independent countries from the start of our period considered *but keeping policy constant*. Note that from 1987 onwards the Flemish share in total debt (expressed as a % of Belgian GDP) (columns 6) starts to decrease (while total debt continues to increase until 1993), as a consequence of the ever growing Flemish primary surplus. Also note that from 1986 onwards the Flemish "caused" share in the interest bill (expressed as a % of Belgian GDP) (columns 7) starts to decrease (while the total interest bill continues to increase until 1990), as a consequence of the decreasing Flemish debt share.

The last but one step is then to calculate regional interest shares not on a "caused" but on a de facto "paid" basis (columns 8), i.e. simply according to regional shares in tax revenue (columns 1). Note that the Flemish interest bill –on a "paid" basis"- starts to decrease later than the Flemish "caused" interest bill, and that it also decreases more slowly.

The difference between the "caused" and "paid" interest shares then equals interregional interest transfers (columns 9). Columns 10 simply express columns 9 as a share of the total annual interest bill. Columns 9 and 10 show that the interest transfers from Flanders -to Wallonia over our entire period, and to Brussels from 1990 onwards- are positive and increasing. At the end of our period, interest transfers from Flanders almost equal the entire interest bill. In nominal euros (not shown in table 8) interest transfers out of Flanders have increased from 9.2 billion in 1995 to 14.6 billion euros in 2002, compared to a total interest bill of 15.9 billion euros in 2002. Note that Brussels is a far smaller region than Wallonia, which partly explains why interest transfers to Brussels are far smaller than those to Wallonia. The large interest transfers from Flanders to Wallonia and Brussels are the combined effect of the rather small part of the interest bill *effectively* paid by Wallonia and Brussels (columns 8), which is due to their rather small (and decreasing) share in total tax revenue (columns 1), and –much more importantly- the very large part of the interest bill Wallonia and Brussels *would* have paid (columns 7) on the basis of their shares in total debt (columns 6).

At the end of our period interest transfers from Flanders are more than double the amount of net transfers calculated the traditional way in a number of studies undertaken during the previous decade.

[82] In columns 6 we also show *total* Belgian debt as a share of Belgian GDP. Remarkably, from 1970 until 1976 the share of debt in GDP steadily decreases despite the steadily worsening primary balance in columns 3. This paradox is explained by the on average relatively strong nominal economic growth and the on average relatively low nominal interest rates over this period. Conversely, columns 6 show that the debt share continued to increase from 1985 until 1993 despite a positive and increasing primary balance (columns 3). This next paradox is explained by the relatively weak nominal economic growth and relatively high nominal interest rates over this period. A final paradox is that from 1994 onwards, the debt share keeps decreasing even though in several years an overall budget deficit was run. This was possible because economic growth made debt as a share of GDP decrease more than that it increased due to successive budget deficits. However, *in nominal euros* (not shown) total Belgian public debt increased steadily over our *entire* period considered.

[83] Hence a region's "caused" interest amount in year t-1 in columns 6 returns as a component of a region's "caused" debt amount in year t in columns 5.

(see e.g. Algoed 2008) These studies assumed that all expenditures were tax funded, i.e. that there was a total primary balance of zero (as correctly pointed out by Algoed (2009 p. 8)). In a nutshell, the difference between interest transfers calculations and traditional transfers calculations is large because regions –at least in Belgium- differ much more in terms of their primary balances as a % of GDP than in terms of the difference between their *shares* in total expenditure and their *shares* in total tax revenue. (see columns 1 until 3 of table 8)

Columns 6 of table 8 show that from 2001 onwards, and due to the successive primary deficits of Wallonia over our entire period –i.e. due to its persistent debt accumulation-, Wallonia's regionalised debt is even larger than total Belgian debt. As a consequence, columns 7 show that the interest bill Wallonia would pay on the basis of its share in total Belgian debt is even larger than the total interest bill. These facts are precisely explained by the facts that at the same end of our period the Flemish share in the public debt has become negative, and hence also the part of the interest bill it would pay on the basis of its share in the total Belgian debt. In other words, the successive primary surpluses of Flanders since 1983 have enabled it to pay down "its" entire debt accumulated before 1983, and have even enabled Flanders to accumulate –intra-Belgian- *savings* from 1998 onwards. These savings have generated –intra-Belgian- interest *income* to the benefit of Flanders rather than an interest *bill* to be paid by Flanders. Still otherwise stated: the savings of Flanders on the one hand and the Walloon debt which is even larger than the total Belgian debt on the other hand are 2 sides of the same coin. The Walloon "surplus debt" is precisely made possible by the Flemish savings. As a consequence, we estimate that shortly after the end of our period, interest transfers have increased to more than 100% of the total interest bill[84].

The sizeable interest transfers we calculate in table 8 may even be an underestimation of real interest transfers. As said above, regional disaggregation of an overall budget boils down to calculating what expenditures and revenues of each individual region would have been *if they would have been independent countries, however keeping policy constant*. Now if Wallonia and Brussels would have become independent countries in 1970 without changing their –unitary Belgian- policy, they would probably have been charged a higher interest rate on their resp. debts than the interest rate Belgium has effectively been charged (and conversely, Flanders may have been charged a lower interest rate on its debt than the interest rate Belgium effectively has been charged).

Finally it is striking from table 8 that absolute and relative interest transfers strongly increase even:

- after 1983; 1983 was at the start of the 2nd subperiod we distinguish, marked by an improving overall primary balance (columns 3), which was the result of the taking effect of the fiscal consolidation measures of the turnaround government that came to power at the end of 1981
- after 1991, the year in which the interest bill (expressed as a % of GDP) starts to decrease (columns 5),
- after 1993, the year in which the combined primary balance of Wallonia and Brussels turns positive (columns 3),
- after 1995, the year in which the total budget balance turns positive in several years (columns 4), which causes the public debt to decrease, at least as a share of GDP (columns 6).

The effects of the 3 latter evolutions are more than compensated by the divergent evolution of regional primary balances (columns 3) that continues after 1991.

[84] As well as that interest transfers have kept on increasing in each year following our sample period, in spite of the steady decrease of the interest bill as a share of GDP in the period that followed our sample period.

Table 8: Calculation of interregional interest transfers in Belgium (1970-2002) (as a share of Belgian GDP (%), unless stated otherwise)

Year	Share in tax revenue (1)				Share in public spending (2)				Primary balance (3)				BB* (4)	IIR** (5)	Public debt (caused) (6)				Interest bill (caused) (7)				Interest bill (paid) (8)				Interest transfers (9)				As a share of interest bill (10)			
	BE	FL	WA	BR	BE	FL	WA	BR	BE	FL	WA	BR	BE	BE	BE	FL	WA	BR	BE	FL	WA	BR	BE	FL	WA	BR	BE	FL	WA	BR	BE	FL	WA	BR
70	100	57	31	13	100	54	35	11	-1.7	-0.1	-2.0	0.4	-5.3	7.4	65.0	36.7	20.0	8.3	3.6	2.0	1.1	0.5	3.6	2.0	1.1	0.5	0.0	**0.0**	0.0	0.0	0	0	0	0
71	100	57	31	13	100	54	35	11	-3.7	-1.2	-2.7	0.2	-7.4	6.2	64.2	35.6	21.1	7.6	3.7	2.1	1.2	0.4	3.7	2.1	1.1	0.5	0.0	**0.0**	0.1	0.0	0	-1	2	-1
72	100	57	31	13	100	54	35	11	-2.5	-0.5	-2.4	0.4	-6.2	6.4	63.9	34.6	22.3	6.9	3.7	2.0	1.3	0.4	3.7	2.1	1.1	0.5	0.0	**-0.1**	0.2	-0.1	0	-2	4	-2
73	100	57	31	13	100	54	35	11	-2.2	-0.3	-2.4	0.6	-5.9	6.7	61.7	32.7	22.9	6.1	3.7	2.0	1.4	0.4	3.7	2.1	1.2	0.5	0.0	**-0.1**	0.2	-0.1	0	-4	6	-3
74	100	57	31	12	100	54	35	10	-4.1	-1.3	-3.2	0.4	-8.0	7.3	57.6	29.8	22.7	5.0	3.8	2.0	1.5	0.3	3.8	2.2	1.2	0.5	0.0	**-0.2**	0.3	-0.1	0	-5	9	-4
75	100	57	31	12	100	54	35	10	-4.7	-1.4	-3.6	0.3	-8.8	7.9	59.3	30.0	24.8	4.5	4.1	2.1	1.7	0.3	4.1	2.4	1.3	0.5	0.0	**-0.3**	0.5	-0.2	0	-7	11	-5
76	100	57	30	12	100	54	35	10	-4.4	-1.2	-3.4	0.2	-8.6	8.0	59.9	29.4	26.4	4.0	4.2	2.1	1.8	0.3	4.2	2.4	1.3	0.5	0.0	**-0.3**	0.6	-0.2	0	-8	14	-5
77	100	58	30	12	100	54	35	11	-3.8	-0.7	-3.3	0.2	-8.5	8.6	63.4	30.2	29.4	3.8	4.8	2.3	2.2	0.3	4.8	2.7	1.4	0.6	0.0	**-0.5**	0.8	-0.3	0	-10	16	-6
78	100	58	30	12	100	55	35	11	-2.9	-0.1	-3.0	0.1	-8.0	8.6	67.0	30.9	32.4	3.7	5.1	2.3	2.4	0.3	5.1	2.9	1.5	0.6	0.0	**-0.6**	0.9	-0.3	0	-12	18	-6
79	100	58	30	12	100	55	35	11	-9.3	-3.5	-5.2	-0.7	-15.0	9.2	70.1	31.2	35.4	3.6	5.8	2.6	2.9	0.3	5.8	3.4	1.7	0.7	0.0	**-0.8**	1.2	-0.4	0	-14	20	-6
80	100	59	30	11	100	55	34	11	-11.2	-4.5	-5.7	-1.0	-17.8	10.2	78.3	34.2	40.0	4.2	6.6	2.9	3.4	0.4	6.6	3.9	2.0	0.7	0.0	**-1.0**	1.4	-0.4	0	-15	21	-6
81	100	59	30	11	100	55	34	11	-10.3	-3.9	-5.4	-1.1	-18.7	11.2	91.6	39.6	46.7	5.3	8.3	3.6	4.3	0.5	8.3	4.9	2.5	0.9	0.0	**-1.3**	1.8	-0.4	0	-16	21	-5
82	100	59	30	11	100	55	34	11	-8.1	-2.5	-4.6	-0.9	-17.5	11.2	101.9	43.5	52.1	6.3	9.5	4.0	4.8	0.6	9.5	5.6	2.8	1.0	0.0	**-1.6**	2.0	-0.4	0	-17	21	-5
83	100	60	30	11	100	55	34	11	-3.6	**0.1**	-3.1	-0.6	-13.5	10.3	112.8	47.3	58.1	7.4	9.9	4.1	5.1	0.7	9.9	5.9	2.9	1.1	0.0	**-1.8**	2.2	-0.4	0	-18	22	-4
84	100	60	29	10	100	55	34	11	-2.6	**0.7**	-2.7	-0.6	-12.7	9.6	116.9	47.6	61.3	8.0	10.1	4.1	5.3	0.7	10.1	6.0	3.0	1.1	0.0	**-2.0**	2.3	-0.4	0	-19	23	-4
85	100	60	29	10	100	55	33	11	-0.3	**2.1**	-1.9	-0.5	-11.3	10.1	121.8	47.9	65.2	8.8	11.1	4.4	5.9	0.8	11.1	6.7	3.2	1.1	0.0	**-2.3**	2.7	-0.3	0	-21	24	-3
86	100	61	29	10	100	55	33	11	**1.4**	**3.2**	-1.3	-0.4	-10.0	9.8	127.2	47.9	69.8	9.6	11.4	**4.3**	6.2	0.9	11.4	6.9	3.3	1.1	0.0	**-2.6**	2.9	-0.3	0	-23	26	-3
87	100	61	29	10	100	55	33	11	**1.3**	**3.2**	-1.4	-0.6	-9.3	8.7	131.6	**47.0**	74.2	10.4	10.6	**3.8**	6.0	0.8	10.6	6.5	3.1	1.1	0.0	**-2.7**	2.9	-0.2	0	-25	27	-2
88	100	61	29	10	100	56	33	12	**2.7**	**4.1**	-0.8	-0.5	-7.6	8.4	131.6	**44.4**	76.2	11.1	10.3	**3.5**	6.0	0.9	10.3	6.3	3.0	1.0	0.0	**-2.9**	3.0	-0.1	0	-28	29	-1
89	100	62	29	9	100	56	33	12	**3.5**	**4.5**	-0.5	-0.5	-7.7	9.3	127.6	**40.2**	76.1	11.4	11.3	**3.5**	6.7	1.0	11.3	7.0	3.2	1.1	0.0	**-3.4**	3.5	-0.1	0	-30	31	-1
90	100	62	29	9	100	56	32	12	**3.8**	**4.8**	-0.4	-0.6	-8.0	9.8	127.7	**37.0**	78.6	12.2	11.8	**3.4**	7.3	1.1	11.8	7.4	3.4	1.1	0.0	**-3.9**	3.9	-0.0	0	**-33**	33	0
91	100	62	29	9	100	56	33	11	**3.0**	**4.3**	-0.7	-0.6	-8.3	9.2	129.8	**34.1**	82.5	13.3	**11.3**	**3.0**	7.2	1.2	11.3	**7.0**	3.2	1.0	0.0	**-4.1**	3.9	0.1	0	**-36**	35	1
92	100	63	29	9	100	56	33	11	**1.3**	**3.4**	-1.2	-0.9	-9.8	9.0	131.2	**31.2**	85.8	14.3	**11.1**	**2.6**	7.3	1.2	11.1	**7.0**	3.2	1.0	0.0	**-4.3**	4.1	0.3	0	**-39**	37	2
93	100	63	29	9	100	56	33	11	**6.4**	**6.4**	0.3	-0.3	-4.7	8.6	138.0	**29.8**	92.2	16.1	**11.1**	**2.4**	7.4	1.3	11.1	**7.0**	3.2	0.9	0.0	**-4.6**	4.2	0.3	0	**-41**	38	3
94	100	63	28	8	100	57	33	10	**6.8**	**6.6**	0.3	-0.1	-2.9	7.3	**136.4**	**24.7**	95.0	16.8	**9.7**	**1.7**	6.7	1.2	9.7	**6.1**	2.7	0.8	0.0	**-4.4**	4.0	0.4	0	**-45**	41	4
95	100	63	28	8	100	57	33	10	**9.4**	**8.2**	1.1	0.1	**0.1**	7.1	**133.4**	**19.0**	97.1	17.4	**9.2**	**1.3**	6.7	1.2	9.2	**5.9**	2.6	0.8	0.0	**-4.5**	4.1	0.4	0	**-49**	45	5
96	100	63	28	8	100	57	33	10	**8.1**	**7.5**	0.6	0.0	**-0.7**	6.8	**130.1**	**11.8**	100.4	18.0	**8.8**	**0.8**	6.8	1.2	8.8	**5.6**	2.5	0.7	0.0	**-4.8**	4.3	0.5	0	**-54**	49	6
97	100	64	28	8	100	57	33	10	**8.7**	**8.0**	0.6	0.2	**0.7**	6.4	**124.7**	**4.9**	101.6	18.3	**8.0**	**0.3**	6.5	1.2	8.0	**5.1**	2.2	0.7	0.0	**-4.8**	4.3	0.5	0	**-60**	53	6
98	100	64	28	8	100	57	33	10	**7.1**	**7.1**	-0.1	0.1	**-0.4**	6.3	**119.3**	**-2.7**	103.5	18.6	**7.5**	**-0.2**	6.5	1.2	7.5	**4.8**	2.1	0.6	0.0	**-5.0**	4.4	0.5	0	**-66**	59	7
99	100	64	28	8	100	57	33	10	**6.7**	**6.7**	-0.1	0.1	**-0.4**	6.1	**115.0**	**-9.5**	105.7	18.9	**7.0**	**-0.6**	6.5	1.2	7.0	**4.5**	2.0	0.6	0.0	**-5.1**	4.5	0.6	0	**-72**	64	8
00	100	64	28	8	100	57	33	10	**5.0**	**5.8**	-0.8	0.0	**-1.8**	6.3	**109.5**	**-16.0**	106.6	19.0	**6.8**	**-1.0**	6.7	1.2	6.8	**4.4**	1.9	0.6	0.0	**-5.4**	4.8	0.6	0	**-78**	69	9
01	100	64	28	9	100	57	33	10	**6.6**	**6.8**	-0.4	0.2	**0.0**	6.2	**108.7**	**-22.2**	111.3	19.7	**6.6**	**-1.3**	6.8	1.2	6.6	**4.2**	1.8	0.6	0.0	**-5.6**	4.9	0.6	0	**-84**	75	10
02	100	64	28	8	100	57	33	9	**7.7**	**7.5**	0.0	0.2	**1.6**	5.7	**106.1**	**-29.7**	115.6	20.2	**6.1**	**-1.7**	6.6	1.2	6.1	**3.9**	1.7	0.5	0.0	**-5.6**	5.0	0.7	0	**-92**	81	11

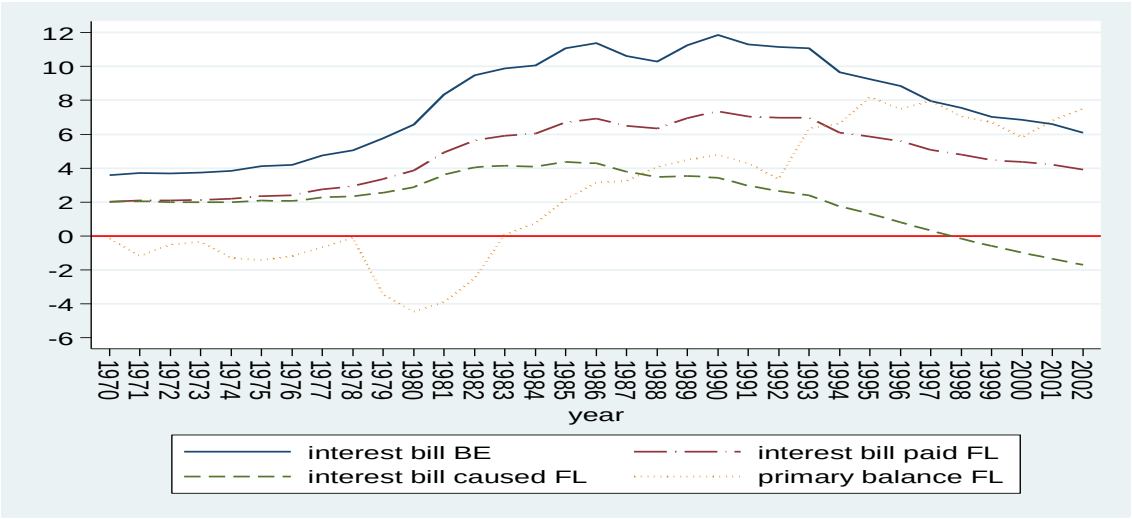
* Belgian federal budget balance (% of GDP)

** Implicit interest rate, i.e. interest bill in year t as a share of Belgian federal public debt in year t-1 (%)

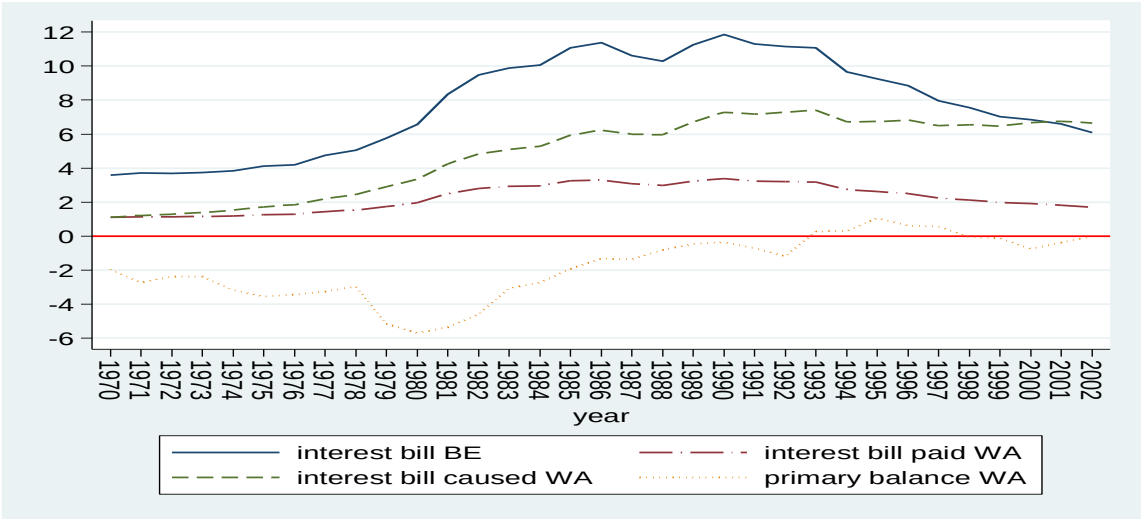
Source: Van Gompel (2004).

Graphs 1 and 2 present the major numbers of table 6, for Flanders and Wallonia resp. We do not show the situation of Brussels graphically, because of its smaller fiscal impact[85]. In graphs 1 and 2, interest transfers are represented by the difference between the dashed line –showing the interest bill caused- and the dashed/dotted line –showing the interest bill paid. As can be seen both in graphs 1 and 2, the gap between both lines widens over time. Graphs 1 and 2 also show increasing interest transfers out of Flanders mirroring increasing interest transfers to Wallonia.

Graph 1: Interest transfers from Flanders compared to the total interest bill (% of Belgian GDP)



Graph 2: Interest transfers to Wallonia compared to the total interest bill (% of Belgian GDP)



Source of graphs 1 and 2: Table 8.

[85] However, *relatively speaking*, the fiscal situation of Brussels may be considered worse than the fiscal situation of Wallonia. Apart from Berlin, Brussels is the only capital city of the EU15 countries not generating net fiscal transfers –calculated the traditional way- towards the rest of the country. Such net fiscal transfers can be calculated using Eurostat data on income taxes and social security contributions paid and social expenditures in cash received. (see http://ec.europa.eu/eurostat/web/regions/data/database) On the contrary, the rest of Belgium –Flanders, to be precise- generates net fiscal transfers *to* Brussels, which also translate into interest transfers *to* Brussels in columns 8 of table 6.

3.4. A concluding attempt at a regional cost-benefit analysis of the Belgian federal public debt

A question that we are left with after calculating interregional interest transfers in table 8 is whether the interest bill that comes with deficit-financed expenditures would render those expenditures more expensive than tax funded expenditures *also for Wallonia and Brussels* -hereafter denoted as FR (Francophone Belgium)- even if to a lesser extent than for Flanders -hereafter denoted as FL. (As Francophone Belgian parties not only collect almost all the votes in Wallonia but also collect many votes in the Brussels region, the 2 relevant regions from a political economics point of view seem FL on the one hand and FR on the other hand.) Are debt-funded expenditures still "cheap" for Wallonia and Brussels *when considered in isolation, i.e. when they are not compared to their cost for Flanders*? Receiving considerable net fiscal transfers resulting from the public debt may still not be sufficient to prevent FR from ending up as a net loser from debt accumulation. In other words: is FR, while benefiting the most on a *gross* basis from debt financing, similar to FL a *net* payer with respect to debt financing? The answer to the latter question could be positive because also FR -each year- pays federal taxes out of which -each year- the interest bill is paid. I.e. *all* Belgian regions may lose on a net basis from debt financing[86]. These questions may be all the more justified because the Belgian debt has been incurred over the previous decades largely to finance social security –i.e. consumptive-expenditures[87].

In table 9 we answer the question if all Belgian regions lose from the Belgian debt. In sum, table 9 shows that FR *also* benefits on a *net* basis from the Belgian debt over our sample period, even if the net costs for FL are much higher than the net benefits for FR. Take table 9a as an example. In table 9a we take as the discount rate the implicit interest rate that held at the end of our sample period, i.e. 5.7% (2002). This is also the *lowest* implicit interest rate that held over our sample period (see column (5) in table 8). In column (1) of table 9a we calculate the joint present value –more precisely the joint value in 2002- of the consecutive primary balances (expressed as a share of Belgian GDP) enjoyed by each of the 3 Belgian regions over our 1970-2002 period (see columns (3) of table 8). Because a primary deficit counts as a benefit for a particular region –as it enables this region to receive more expenditures or to pay less taxes-, we show an accumulated primary *deficit* for a particular region with a *positive* sign in column (1), and vice versa. In column (2) of table 9a we calculate the joint present

[86] At least when we disregard possibly differing time preferences between Belgian regions.

[87] At first sight, Belgian debt accumulation hence looks comparable to 2 ex-classmates who take a loan of 100 together from a bank –that comes say with a constant annual interest rate of 10% (for the sake of simplicity)- to pay for their annual get-together in a restaurant. Suppose the poorer one of the 2 could spend 60 of it on his dinner and the richer one 40. Suppose the friends would agree that each of the next 50 years to come the richer one would pay an interest amount of 6 and the poorer one only 4. Even the behaviour of the poorer friend could hardly be considered rational. The irrationality of the couple of friends –and of its individual members- would only increase if during *each* of these 50 years they would continue to take a *new* loan of 100 together to pay for their annual restaurant get-together, as their interest bill would increase each year. The major victim of the accumulated get-togethers would probably be the poorer friend, because he can possibly much less afford to borrow with a view to funding restaurant visits, even if he enjoys more than half of the benefits and shoulders less than half of the costs.
However, in case debt-financed expenditures that benefited FR would have been invested "productively", i.e. would have generated a higher rate of return than the implicit interest rate paid on the debt by FR, the above reasoning would not hold anymore. (Also "wise" social security –i.e. "consumptive"- spending could have generated such a rate of return.) A high enough rate of return for FR could even have made FL a net beneficiary of the Belgian debt, as high economic growth in Francophone Belgium evidently means also higher economic growth in FL (or at least lower transfers from FL to FR). In sum, public debt may be economic growth enhancing, while in our calculations above we have disregarded the "benefit side" of public debt.

value –more precisely the joint value in 2002- of the consecutive interest payments shouldered by each of the 3 Belgian regions over our 1970-2002 time period[88] (see columns (8) of table 8). Because an interest payment obviously counts as a cost for a particular region, we show accumulated interest payments for a particular region with a *negative* sign in column (2). The addition of column (1) to column (2) then gives the net cost of debt funding (column (3)). We notice that the gross costs of debt outweigh its gross benefits for each region, except for Wallonia. That the net benefits of debt for Wallonia are higher than the net costs for Brussels make that FR taken together incurs a net benefit of the Belgian debt, despite the net cost of Belgian debt for Brussels. However, the net benefits of FR are far smaller than the net costs for Flanders.

Table 9: 2002 present values of net benefits (in % of Belgian GDP) of the Belgian public debt per Belgian region

a) Taking the lowest implicit interest rate over our sample period as the discount rate: 5.7% (2002)

	PV of primary balances (1)	PV of interests paid (2)	(3) = (1)+(2)	Debt stock to repay (4)	(5) = (3)+(4)
FL	-75%	-294%	**-370%**	-63%	**-433%**
WA	224%	-141%	83%	-27%	56%
BR	11%	-49%	-38%	-8%	-46%
FR	235%	-190%	**45%**	-35%	**10%**
BE	160%	-485%	-324%	-98%	-423%

b) Taking the average implicit interest rate over our sample period as the discount rate: 8.2%

	PV of primary balances (1)	PV of interests paid (2)	(3) = (1)+(2)	Debt stock to repay (4)	(5) = (3)+(4)
FL	-49%	-422%	**-471%**	-63%	**-534%**
WA	393%	-204%	189%	-27%	162%
BR	11%	-71%	-60%	-8%	-68%
FR	405%	-276%	**129%**	-35%	**94%**
BE	355%	-697%	-342%	-98%	-440%

c) Taking the highest implicit interest rate over our sample period as the discount rate: 11.2% (1981 and 1982)

	PV of primary balances (1)	PV of interests paid (2)	(3) = (1)+(2)	Debt stock to repay (4)	(5) = (3)+(4)
FL	31%	-668%	**-637%**	-63%	**-700%**
WA	781%	-327%	454%	-27%	427%
BR	4%	-116%	-112%	-8%	-120%
FR	785%	-443%	**342%**	-35%	**306%**
BE	816%	-1111%	-295%	-98%	-393%

The positive number for FR in column (3) of table 9a can be interpreted as follows: an annual rate of return of only 5.7% -i.e. equalling the historically low implicit interest rate of 2002 on the Belgian debt- generated by the extra expenditures made possible by the successive primary deficits of FR would have been sufficient to make it profitable for FR to strive for these successive primary

[88] We are aware that interest payments at the beginning of our period predominantly stem from debt –and hence primary deficits- incurred before the 1970-2002 period. Therefore, we only consider interest payments on public debt newly incurred since 1970, i.e. we assume that public debt was zero in 1970.

deficits[89]. We can easily calculate that at a discount rate of 3.0% the present value of net benefits of the Belgian public debt for FR becomes zero, i.e. at a discount rate of 3.0% it would no longer have been profitable for FR to strive for primary deficits.

Table 9a shows that it is rational for FR to push for extra expenditures, even when they are debt financed, and even if the net benefit of Belgian deficit financing for FR is relatively limited while generating a very heavy bill to be shouldered by FL, which is by far the largest region of Belgium, and which has veto power over any federal policy[90]. What is even more: precisely because they are debt-financed –and because their costs are therefore politically less visible-, FR politicians may find that generating debt-financed extra expenditures is more feasible than generating tax-funded extra expenditures.

To obtain the net cost of the Belgian public debt per region, it could be argued that we should add to column (2) the part of the total debt stock in 2002 that would have to be repaid per region if this debt stock would be fully repaid in 2002 (column (4))[91]. Similar to the interest payments of column (2), we use a region's share in tax revenues as the criterion to distribute the debt stock across Belgian regions, and we give a negative sign to debt stock repayments as these should be considered as costs to be compared to the benefits of running a primary deficit (column (1)). The sum of columns (3) and (4) is shown in column (5). We notice from column (5) that FR *just* keeps benefiting from the Belgian debt when also considering the fact that the Belgian debt itself eventually may have to be repaid (and that -obviously- the cost of Belgian debt for FL is even higher when considering this fact).

For Belgium as a whole, additionally taking into account debt stock repayment only makes the outcome of our cost-benefit analysis change from considerably negative to even more negative. One may be puzzled by this overall heavily negative outcome, as one may have expected that a net loss from Belgian debt incurred by one region may have more or less corresponded to a net benefit incurred by the other regions. The cause of the large overall net loss is that we only consider *primary* balances at the benefits side, rather than *overall budget* balances. E.g. for FR, the overall budget deficit has always been larger than the primary budget deficit. Considering overall budget deficits rather than primary budget deficits at the benefits side would lead to an even more positive outcome for FR and a less negative outcome for FL. But arguably, it would have been a mistake to consider the regionalised interest payments –which distinguish the overall budget deficit from the primary deficit- as a benefit of the Belgian public debt. Another explanation of the overall net loss may be provided by the high to very high implicit interest rates that held with respect to the Belgian public debt in many years of our sample period. (see column (5) in table 8)

With respect to the question of including debt stock repayment or not, we could as well argue that a rational politician's horizon may not be long enough to bring into consideration debt stock repayment, as a rational politician may have an incentive to postpone debt stock repayment eternally[92]. The 1st column of columns (6) in table 8 shows that as a share of GDP Belgian debt started steadily to

[89] As most of these expenditures consisted of social security expenditures, i.e. "consumptive" expenditures, one may wonder if such a rate of return has been obtained.

[90] Velasco (1997 p. 15) theoretically comes to a result that is overall even worse than our outcome: groups borrow, purely for strategic reasons, and this results in lower utility for *all* players.

[91] Similarly to interest payments in column (2), we only take into account in column (4) the public debt incurred after 1970. For this reason, the total debt stock in tables 9 is slightly smaller than the total debt stock in column (6) of table 8.

[92] Long enough postponement of repayment would make the present value of debt stock repayment equal to zero.

decrease from 1994 onwards, when Belgium needed to start qualifying for Eurozone accession. However, *in nominal euros* Belgian debt has always kept increasing. Shortly after Eurozone accession the overall primary surplus –shown in the 1st column of columns (3) of table 8- started decreasing (not shown in the 1st column of columns (3), as our dataset ends in 2002). Since the outbreak of the economic crisis in 2008, Belgium is back in a situation of an overall primary deficit (not shown in the 1st column of columns (3) either). Another argument to disregard the impact of debt stock repayment is that in recent years it has been argued that FL should take over the entire Belgian debt stock in case of a breakup of Belgium, as it is only in FL that such a breakup is being advocated by some stakeholders. The latter arguments are reasons to stop our calculations after column (3), i.e. not to take into account the debt stock.

As a sensitivity analysis, tables 9b and c repeat the calculations of table 9a using a higher discount rate, resp. the *average* implicit interest rate over our sample period and the *highest* implicit interest rate over our sample period. The conclusion of this sensitivity analysis is not fundamentally different from our analysis in table 9a: the benefits of Belgian debt for FR -and –consequently- the costs of Belgian debt for FL- only *increase* with an increasing discount rate. This increase of costs and benefits is due to the fact that a higher discount rate attaches more weight to the very large primary deficits –mainly having benefited Wallonia- of the end of our 1st subperiod, as well as to the very large interest payments –mainly having been shouldered by FL- of our 2nd subperiod.

4. Conclusion

Our calculations above suggest that there is a geographic dimension to the political economy of debt financing in Belgium, next to other political economics causes of Belgian debt accumulation, and next to purely economic causes of Belgian debt accumulation[93]. The diverging shares of the Belgian regions in federal tax revenues and in federal public expenditures make Flanders pay disproportionately for tax funded public expenditures *and* pay even more disproportionately the interest bill that is the consequence of the considerable debt financing of federal public expenditures since 1970. Hence calculating interregional net transfers in Belgium is less futile than it seems: the differing regional costs of federal debt accumulation in Belgium may be an explanation of the Belgian history of high deficits and high debts since the beginning of the 1970s. We do not know of any other research on any other country that explores this possibility.

The implications of our calculations strongly differ for politicians elected in Flanders and for politicians elected in Wallonia and Brussels. Given the resp. shares of the Belgian regions in federal tax revenues and public expenditures, politicians elected in the Walloon and the Brussels region seem to have an interest in keeping on "expanding the federal budget constraint" -i.e. in eliciting extra expenditures- *in particular* if the extra expenditures are funded by borrowing rather than by tax revenue[94]. Hence, politicians from these regions should maximise the federal deficit (at least as long as the size of the federal deficit does not cause visible macroeconomic damage to their region).

[93] It has e.g. been empirically demonstrated that small open economies are characterised by a larger public sector, which in turn increases the risk of a higher public debt.

[94] This interest should be understood here from a political-economy perspective, i.e. it should be understood as the "*career* interest" of politicians representing net expenditure receiving jurisdictions in the federal government, as those politicians would be able to claim the credit for the extra expenditures. The *welfare* effects of these extra expenditures are a different matter altogether. In theory, a budget deficit may be counter-cyclical

For Flemish politicians the incentives seem to be the exact opposite of the incentives of Walloon and Brussels politicians. Flemish politicians should even favour the policy option of closing the federal budget deficit with extra federal tax revenue *exclusively generated in Flanders* over keeping on running a federal budget deficit. Federal expenditures disproportionately benefiting Wallonia and Brussels are far "cheaper" for Flanders if tax funded than if debt funded.

The implications seem similar for politicians from other countries with a high central government debt and considerable disparities in the extent to which its resp. regions provide tax revenues to the central government, resp. benefit from central government spending, such as Italy. However, as in Italy political parties are not geographically split, unlike in Belgium, geographically differing deficit incentives seem to apply less to Italy as an explanation of the size of the central government debt.

A concluding question then is why –given their veto power within the setting of the Belgian Federal government- Flemish politicians have not prevented (or been able to prevent) repeatedly large deficits over our period studied. A first reason may have been that coalition members having an interest in *preventing* a budget deficit may generally face a more difficult task anyhow than coalition members having an interest in *eliciting* a debt-funded expenditure increase (or tax decrease). A 2nd reason may have been that the Belgian federal debt game may not be a –symmetric and simultaneous- "prisoner's dilemma", but rather a –asymmetric and sequential- game with a first mover advantage, such as the also rather well-known "entry deterrence game". (Rasmusen 2006 p. 14-16 and 112) If Francophone Belgian politicians thought themselves entitled to generate a budget deficit –e.g. because over our sample period Francophone Belgium was poorer, more unemployed, and smaller ("too small to fail"; see also Inman 2003 p. 47 and Rodden 2005 p. 194)-, they may have "filled up the room for a budget deficit" during their first move. In the wording of the "entry deterrence game", the new firm (Francophone Belgian politicians) enters, and subsequently stays in the business (Francophone Belgian politicians' constant relative indiscipline). This may have happened because the new firm (Francophone Belgian politicians) may have known that the incumbent firm (Flemish politicians) would always collude/cooperate (Flemish politicians' constant relative discipline). Indeed, its threat to fight the new entrant –by being indisciplined as well- may have been non-credible, because fighting would have damaged not only the new entrant but also the incumbent (Flanders "too big *to be prevented from* failing")[95].

A 3rd reason for the behaviour of Flemish politicians could be that the part of taxation generated in Flanders to pay the interest bill may generally have been spread over the *entire* Flemish population, while individual governing politicians elected in Flanders may have been able to *target to their constituents* part of the extra expenditures (or targeted tax decreases) *in Flanders* made possible by

---

and investment expenditures are suited to be financed by borrowing. However, the extra transfers generated by past Belgian deficits may also be welfare-reducing. E.g. Algoed and Persyn (2009) find an economic growth reducing and inequality increasing effect of interregional net fiscal transfers in European countries. Knight (2004) finds with respect to pork-barrel spending in US districts that it is inefficient and that it generates a large deadweight loss.

[95] A natural question to ask then is if Francophone Belgian politicians also *knew* that Francophone Belgium would disproportionately benefit from the extra expenditures generated by means of running a budget deficit. As Flanders and Francophone Belgium considerably differ in terms of demographic, social and economic characteristics, there indeed seems some scope for Francophone Belgian resp. Flemish politicians who are part of the *same* central government coalition to be fiscally indisciplined "in one's own interest", i.e. so that extra expenditures or tax cuts predominantly benefit Francophone Belgian resp. Flemish voters. An example is the finding of "Hoge Raad voor Financiën", a federal fiscal policy watchdog, that the major 2001 income tax cut –overseen by the Francophone Belgian liberal-conservative minister of finance Didier Reynders- mainly benefited inhabitants of the Walloon region. (HRF 2010)

debt financing[96]. These constituents mostly belong to a *particular social group* and/or only live in a *particular geographic part* of Flanders. Most politicians elected for the federal parliament over our period studied were able only to collect votes within an electoral district that was considerably smaller than the region this district was part of[97]. For Belgian examples of political economy motivated distribution of public expenditures to particular electoral districts only, see Jennes and Persyn (2015) and Jennes (2014b).

A 4th intra-Flemish explanation of a Flemish deficit incentive even given that Flanders incurs an aggregate cost –instead of a benefit- from the Belgian federal debt may be that some Flemish parties –in particular the Flemish liberal-conservatives- may have felt less sure of remaining in power than other parties, which would have given them an incentive to run a deficit. A 5th explanation may be the prisoner's dilemma type of setting suggested by Roubini and Sachs (1989 p. 924): perhaps each individual Flemish party prefers a balanced budget, but in the absence of strong coordination *among* Flemish parties, each of them may have had an incentive to protect and expand "its" part of the budget. A final possible explanation is that Flemish parties may have used their collective veto power to trade a worsening of the budget balance –as would have been requested by the Francophone Belgian parties- for a concession made by the Francophone Belgian parties in another policy area[98].

References

Abafim, "Financiële transfers tussen de Belgische gewesten", 2004.

Alesina, A. and A. Drazen, "Why are stabilisations delayed?". NBER working paper, 1989.

Alesina, A. and R. Perotti, "The political economy of budget deficits". NBER working paper, 1994.

Alesina, A. and G. Tabellini, "A positive theory of fiscal deficits and government debt in a democracy". NBER working paper, 1987.

Alesina, A. and G. Tabellini, "Positive and normative theories of public debt and inflation in historical perspective", European Economic Review 39 (1992): 337-344.

Algoed, K., "Interregionale fiscale stromen. Een toepassing van het 'balance sheet federalism' voor het jaar 2005", Steunpunt Fiscaliteit en Begroting, 2008.

Algoed, K., "Interregionale fiscale stromen en het verrekenen van de rentelasten", 2009.

Algoed, K., and D. Persyn, "Interregional redistribution, growth and convergence". Vives Discussion Paper, 2009.

[96] Moreover, the tax revenue generated in Flanders to pay the interest bill may generally have been spread unequally over the Flemish population, so that particular Flemish constituencies or electoral districts may pay a larger part of the interest bill than other.
This may have made it impossible to find 2/3 of the Dutch-speaking group in the federal parliament ready to veto legislation that would increase the deficit.

[97] Exceptions were those few politicians running for the Belgian Senate and also to some extent those few politicians running for the House district of Brussel-Halle-Vilvoorde.

[98] This would be reminiscent of the common thread that a number of observers have tended to perceive in Belgian "state reforms": as an exchange of an increase in decentralisation –as requested by Flemish parties- for an increase in federal grants to those subcentral governments that were dominated by Francophone Belgian parties.

Ashworth, J., B. Geys, and B. Heyndels, "Government weakness and local public debt development in Flemish municipalities", International tax and public finance 12 (2005): 395-422.

Banzhaf, J., "One man, 3.12… votes: a mathematical analysis of the electoral college", Villanova Law Review 13 (1968): 304-346.

Baskaran, T., "Soft budget constraints and strategic interactions in subnational borrowing: evidence from the German states", Journal of Urban Economics 71 (2012): 114-127.

Baskaran, T., "Coalition governments, cabinet size, and the common pool problem: evidence from the German states", European Journal of Political Economy 32 (2013) : 356-376.

Besley, T., "Principled agents", 2006.

Bird, R. and F. Vaillancourt, "The interregional incidence of central budgets in federations. Some evidence from Canada", Public budgeting & finance 27 (2007): 1-19.

CERPE, "Transferts interrégionaux et soldes nets à financer régionaux belges". Namen, FUNDP, 2011.

Commissie Van de Voorde, "Verslag van de Commissie voor de studie van de methodologie gebruikt bij de analyse van de interregionale transfers", 2006.

Decoster, A. and D. Verwerft, "Solidarity: regions or persons? Some calculations to illustrate the "by-product" status of interregional transfers", in: Re-Bel Book 1, "On the interaction between subsidiarity and interpersonal solidarity" (2009): 45-53.

de Haan, J., and E. Sturm, "Political and economic determinants of OECD budget deficits and government expenditures. A reinvestigation", European Journal of Political Economy 13 (1997): 739-750.

Denkgroep "In de Warande", "Manifest voor een zelfstandig Vlaanderen in Europa", 2005.

De Tijd, "Reynders spreekt zich uit voor uitbreiding notionele intrestaftrek", 20 February 2009.

Dottermans, G., " Financiële stromen tussen de gewesten in België 1955-1975", Documentatieblad Ministerie van Financiën 57 (1997).

Dubay, C., "Federal tax burdens and expenditures by state. Which states gain most from federal fiscal operations?" Tax foundation, 2006

Huber, G., M. Kocher and M. Sutter, "Government strength, power dispersion in governments and budget deficits in OECD-countries. A voting power approach", Public Choice 116 (2003): 333-350.

Inman, R., "Transfers and Bailouts: Enforcing Local Fiscal Discipline with Lessons from U.S. Federalism", in: Rodden, J., G. Eskeland, and J. Litvack, "Fiscal Decentralization and the Challenge of Hard Budget Constraints". The MIT Press, 2003.

Jennes, G., "Publieke transferten tussen de Belgische gewesten 2007-2011". KULeuven, Vives, 2014a.

Jennes, G., "The allocation of grants to Brussels municipalities: the effect of partisan alignment". KULeuven, Vives Discussion Paper, 2014b.

Jennes, G. and D. Persyn, "The effect of political representation on the geographic income distribution in Belgium: evidence using Belgian data", European Journal of Political Economy 37 (2015): 178-194.

Jennes, G., "The Law of 1/n for the supra-local governments of Belgium: does "more politicians" mean "more public spending"?" KULeuven, Vives discussion paper, 2015.

Khemani, S., "Party politics and fiscal discipline in a federation: Evidence from the states of India", Comparative Political Studies 40 (2007): 691-712.

Knack, "Volleerde struisvogel", 14 January 2009.

Knight, B., "Parochial interests and the centralized provision of public goods", Journal of Public Economics 88 (2004): 845-866.

Lago-Peñas, S., A. Sacchi, and P. Simon-Cosano, "Who honor the rules of federalism? Party system nationalisation and fiscal performance". Universida de Vigo, GEN working paper, 2014.

Lizzeri, A., "Budget deficits and redistributive politics", Review of Economic Studies 66 (1999): 909-928.

Luykx, T., "Politieke geschiedenis van België". Elsevier, 1985.

NBB, "Intergewestelijke overdrachten en solidariteitsmechanismen via de overheidsbegroting," Economisch Tijdschrift (2008): 99-120.

NBB, "Verslag 2009 - Economische en financiële ontwikkeling", 2010.

OECD, "How regions grow. Trends and analysis", 2009.

Perotti, R., and Y. Kontopoulos, "Fragmented fiscal policy", Journal of Public Economics 86 (2002): 191-222.

Persson, T. and L. Svensson, "Why a Stubborn Conservative Would Run a Deficit. Policy with time-inconsistent preferences", Quarterly Journal of Economics 104 (1989): 325-345.

Persson, T. and G. Tabellini, "Political economics". MIT Press, 2000.

Persson, T. and G. Tabellini, "The economic effects of constitutions". CES, 2003.

Persyn, D. and W. Torfs, "A gravity equation for commuting with an application to estimating regional border effects in Belgium", Journal of Economic Geography (2015): 1–21.

Pettersson-Lidbom, P., "An empirical investigation of the strategic use of debt", Journal of Political Economy 109 (2001): 570-583.

Pettersson-Lidbom, P., "Testing political agency models". University of Stockholm Working Paper, 2006.

Pettersson-Lidbom, P., "Dynamic commitment and the soft budget constraint: an empirical test", American Economic Journal: Economic Policy 2 (2010): 154-179.

Rasmusen, E., "Games and Information: An Introduction to Game Theory". Blackwell Publishers, 2006.

Res Publica, "Politiek Jaarboek", several years.

Rodden, J., "Hamilton's Paradox: The Promise and Peril of Fiscal Federalism". MIT, 2005.

Roubini, N. and J. Sachs, "Political and economic determinants of budget deficits in the industrial democracies", European Economic Review 33 (1989): 903-938.

Sorribas-Navarro, P., "Bailouts in a federal system: evidence from Spain", European Journal of Political Economy 27 (2011): 154-170.

Van Gompel, J., "Een denkoefening over regionale schuldsplitsing in België", 2004.

Van Rompuy, P. en V. Bilsen, "10 jaar financiële stromen tussen de gewesten in België", Leuvense Economische Standpunten nr. 45, 1988.

Velasco, A., "A model of endogenous fiscal deficits and delayed fiscal reforms". NBER working paper, 1997.

Volkerink, B. and J. de Haan, "Fragmented government effects on fiscal policy. New evidence", Public Choice 109 (2001): 221-242.

Chapter 3: The geography of incentives to run a federal budget deficit in Belgium[99]

Abstract

Political parties in Belgium are split along Flemish-Francophone lines, and Flanders enjoys relatively less federal expenditures and pays relatively more federal taxes than Francophone Belgium. Also, as the federal debt is serviced out of federal tax revenues, Flanders bears most of the cost of debt servicing. We test for any resulting geographically differing budget deficit incentives by investigating the difference between the effect of an increase in net formula-based transfers on Flemish resp. Francophone Belgian Federal government parties' vote share.

We do not find a consistently significantly positive effect of transfers on votes in Francophone Belgium, and we find weak evidence of a significantly smaller effect of transfers on votes in Flanders. These findings may imply that Francophone Belgian voters overestimate the cost of extra transfers, and that the opposite holds for Flemish voters. Alternatively, Francophone Belgian parties possibly overestimate the electoral rewards that extra net transfers bring them, while Flemish parties possibly correctly estimate the –lack of- relative electoral punishment that extra transfers bring them. This may explain the continuous federal budget deficits and the high federal public debt in Belgium.

Keywords: distributive politics; income redistribution; fiscal transfers; public debt; elections; political representation; fiscal federalism

[99] I wish to thank Thushyanthan Baskaran for the research idea, and Ivo Bischoff, Filip Bossier, Erik Buyst, Rik Daems, Guido De Bruyne, Karen De Meyst, Andrew Eggers, Hilde Geeraers, Danny Geerts, Jordi Jofre-Monseny, Joep Konings, Bart Maddens, Petros Milionis, Klaas Staal, David Stadelmann, Wouter Torfs, Bas van Aarle, Robrecht Vandendriessche, Tom Van Puyenbroeck, Jakob Vanschoonbeek, Pim Verbunt, Wouter Vermeulen, Hye Young You, my colleagues at Vives, participants at the 2015 KULeuven Economics Doctoral Workshop, the 2015 EPCS annual meeting in Groningen, the 2015 ZEW public finance conference in Mannheim, and the 2015 APSA annual meeting in San Francisco for their advice on the empirical strategy. I wish to thank the Belgian federal Ministries of Employment and of Pensions, Statistics Belgium, and Jo Buelens and Kris Deschouwer (VUB) for data provision.

1. Introduction

In what follows we will empirically investigate the possibly geographically divergent incentives of politicians to run a federal budget deficit in Belgium, which are suggested by our geographic attribution of the interest payments on the federal debt. Table 1 below shows differences between the interest bill on the Belgian federal debt that annually falls due according to *in which Belgian region this federal debt originated* and the interest bill that falls due according to *which Belgian region generated the federal tax revenue to pay this interest bill* over the 1970-2002 period, as well as for the final year of this period. The 1970-2002 period is the period for which we found data that enabled us to allocate Belgian federal public expenditures and federal tax revenues –and hence also the (primary) deficit- regionally.

Because since the 1970s political parties in Belgium are split along French-speaking/Flemish lines[100], we only consider 2 regions in table 1: Flanders and Francophone Belgium, thereby lumping the predominantly French-speaking regions of Wallonia and Brussels together[101]. A 2nd motivation to only consider 2 regions is that in 1970 a change of the constitution took place which organised the Federal government in a confederal way:

1) the legislative was divided into a "Dutch-speaking group" and a "French-speaking group", and any 2/3 of members of parliament of any group are since then entitled to block any law from being approved;
2) the executive –which decides by consensus- since then needs to consist of an equal number of ministers belonging to either of both parliamentary groups

Table 1: Differences between annual interest bill on the federal debt *caused and paid*, and resulting annual fiscal transfers per Belgian region (in % of Belgian GDP) (1970-2002)

	Belgium	**Flanders**	**Francophone Belgium**
Average interest bill caused	7.8%	2.1%	5.6%
Interest bill caused (2002)	6.1%	-1.7%	7.8%
Average interest bill paid	7.8%	4.8%	3.0%
Interest bill paid (2002)	6.1%	3.9%	2.2%
Average fiscal transfers due to the interest bill	0.0%	**-2.6%**	**2.6%**
Fiscal transfers due to the interest bill (2002)	0.0%	**-5.6%**	**5.6%**
Average federal budget balance	**-7.3%**		
Average federal public debt	**102.5%**		

Source: Jennes 2014a

[100] From a worldwide perspective, such a split seems rather unique. E.g. in their study of the impact of the regionalisation of party systems on fiscal performance, Lago-Peñas e.a. (2014) characterise Belgium as a case of extreme party system regionalisation, together with Switzerland, the Philippines, India, Pakistan, Thailand and Colombia.

[101] Voters of the Brussels Capital Region overwhelmingly voted for Francophone Belgian parties over our sample period. E.g. in the 2007 federal elections the Flemish parties that won seats in the federal parliament obtained 8.9% of the total vote in the Brussels Region, while the Francophone parties that won seats in the federal parliament obtained 68.9% of the total vote in the Brussels Region.

Table 1 shows that Flanders on average has shouldered a considerably larger share of the interest bill (= "interest bill paid") than warranted by its share in the extra federal expenditures made possible by the consecutive federal budget deficits (= "interest bill caused") over our sample period. This difference has caused an average annual implicit net fiscal transfer through the federal budget process from Flanders to Francophone Belgium of 2.6% of Belgian GDP. Moreover, this transfer has steadily increased over our sample period, up to no less than 5.6% of Belgian GDP in 2002 –i.e. up to no less than 92% of the total federal interest bill in 2002[102]. This steady increase has mainly been caused by a number of federal budget consolidation efforts since the 1981 federal elections, efforts that have improved the implicit federal primary balance of Flanders more than the implicit federal primary balance of Francophone Belgium. (for more details, see Jennes 2014a) More in particular, successive implicit federal primary surpluses of Flanders since 1981 contrast with continuing implicit federal primary deficits in Francophone Belgium. These successive Flemish implicit federal primary surpluses have rendered the interest bill caused by Flanders even *negative* from 1998 onwards according to our calculations (resulting in a negative figure for Flanders in the 3rd row of table 1). I.e. from 1998 onwards Flanders implicitly *generated interest revenue* instead of causing an interest bill. The resulting increasing annual fiscal transfer from Flanders to Francophone Belgium may have provided Francophone Belgian politicians with an incentive to make the federal budget run a deficit, in contrast to Flemish politicians, who seem to have had an incentive in running a budget surplus, so as to be able to start reimbursing the public debt[103].

We will investigate the geography of budget deficit incentives in Belgium by estimating if the effect of *net* fiscal transfers –i.e. federal expenditures received minus federal taxes paid-[104] on popularity of the Federal government differs between Flanders and Francophone Belgium, as *net* taxes paid and expenditures received per geographic area measure this area's contribution to the federal budget balance[105]. In contrast to the existing political economy literature on debt, our investigation concerns an empirical test of (1) the central government's debt game (2) between different regions (3) represented within one and the same central government. Moreover, in contrast to the existing distributive politics literature, we hypothesise that voters from the Belgian region that de facto pays most of the debt service burden, i.e. Flanders, *punish* –at least relatively speaking, i.e. compared to Francophone Belgian voters- *the incumbent parties with less votes for increasing transfers*. The transfers we investigate are formula-based[106] *and* occurring between the Federal government and its

[102] It is sometimes argued that since Flanders is the richest region of Belgium, interest payments on the Belgian federal public debt would disproportionately benefit inhabitants of Flanders, as the latter would disproportionately hold Belgian federal government bonds. However, it could well be that only a minority of inhabitants of Flanders hold Belgian Federal government bonds. Moreover, those inhabitants would have looked for an at least as profitable investment should they not have invested in Belgian federal bonds.
The federal Ministry of Finance did not respond to our request for data on the distribution of Belgian federal bonds over Belgian citizens according to region.

[103] In fact, a sufficient condition for such an incentive already seems the fact that federal taxes are disproportionately paid by Flemish citizens, (unrealistically) assuming that all Belgian regions would have benefited from the extra expenditures made possible by a budget deficit in proportion to their share in Belgian population. I.e. we even do not need the rather spectacular figures of table 1 to assume that Francophone Belgian politicians have more of an incentive to run a federal deficit than Flemish politicians.

[104] Note that the –explicit- transfers concept we define here differs from the –implicit- transfers concept we use in table 1 above.

[105] Of course we cannot estimate the possibly geographically differing impact of the *aggregate federal budget balance itself* on popularity of the Federal government, because of course the aggregate federal budget balance *does not vary* between geographic areas of Belgium.

[106] In contrast to "discretionary", "formula-based" needs to be understood as treating each citizen according to the same formula, i.e. benefiting or costing those citizens to exactly the same extent provided that those citizens

citizens. Therefore, such relative punishment could be due to Flemish voters fearing that an increase of transfers to Flemish citizens would not only go hand in hand with an increase of transfers to Francophone Belgian citizens –an increase that is possibly even larger, as the formulas behind the transfers we consider favour poorer citizens-, but would also go hand in hand with an increase in the public debt as a result of these increases.

Unfortunately, a geographic disaggregation of the *entire* Belgian federal budget (revenues + expenditures) does not exist[107], let alone over a sufficient number of years. However, in its annually updated "Regional accounts"[108], the Belgian central bank NBB provides for every year over the 1995-2011 period[109] a geographic disaggregation down to the level of the 43 Belgian "administrative arrondissements" (or districts)[110] with respect to following important *federal* revenue and expenditure categories: income taxes paid[111] (-), -mandatory- social security contributions paid (-), and social transfers received[112] (+). Hence these "Regional Accounts" have 2 important advantages with respect to our research question: (1) they are the only existing disaggregation of (part of) federal taxing and spending down to the Belgian district level; (2) they disaggregate an important part of government taxing and spending that is directly happening between the government and the citizen.

The social transfers variable included in the "Regional accounts" comprises all major social transfer categories in Belgium: pensions, unemployment benefits, child allowances, work disability allowances, allowances for the handicapped, and subsistence allowances[113]. The 3 transfers categories we consider –i.e. social spending in cash, social security contributions, and income tax payments- are generally very important in OECD countries. E.g. in 2010, income taxes on average amounted to 8.4% of GDP in OECD countries, while social security contributions on average amounted to 10.1% of gross wage earnings and social expenditures on average amounted to 22.1% of GDP (see www.oecd.org). In Belgium social expenditures even amounted to 29.5% of GDP in 2010. These 3 transfer categories arguably constitute the core of the Belgian "welfare state", and can be argued to be visible tax and expenditure categories, as they constitute direct and sizeable transfers from and to individual citizens.

---

are in the same economic or demographic situation. "Formula-based" should not be understood as "impossible for politicians to change".

[107] In contrast to e.g. the US situation, as witnessed by Dubay (2006).

[108] http://www.nbb.be/doc/dq/n/dq3/NNR.pdf

[109] Note that the "Regional accounts" data cover a different period than the period on which our calculations resulting in table 1 are based. We regret the lack of overlap between these 2 periods, while maintaining that the finding in table 1 is a relevant motivation for our regression analysis below concerning the 1995-2011 period.

[110] In Belgium, the "arrondissement" or district level is a level that lost its policy meaning decades ago, as opposed to the regional and the community subcentral government levels. However, several of these districts were in effect federal *electoral* districts during part of our sample period, i.e. before the electoral districts merger of 2002. This meant that members of the federal House of Representatives could only collect votes in their district of official residence. Annex 1 shows a map of the 43 "arrondissements" (or districts) of Belgium.

[111] Over our sample period, a small part of the authority to levy income taxes was devolved to the 3 Belgian regions. Mainly the Flemish region made use of this authority, by cutting income taxes slightly for inhabitants of the Flemish region in the 2007-2009 period only. We assume that these tax cuts have a negligible influence on our regression results below.

[112] Only social transfers *in cash* are included in our data, excluding social expenditures *in kind*, such as education and health care, for which the "Regional accounts" do not collect data.

[113] The latter 2 subcategories are not formally part of the Belgian social security system.

We label the sum of these 3 important and geographically disaggregated fiscal categories "(net) transfers". Expressed by means of a simple formula:

$$(net)\ transfers_{it}\ (-)$$

$$= social\ transfers_{\ it}\ (+) + social\ security\ contributions_{it}\ (-) + income\ tax\ payments_{it}\ (-)$$

with *i* denoting a particular district and *t* denoting a particular year.

While most of the distributive politics literature investigates the political economy causes and effects of (1) *discretionary* (2) *intergovernmental grants or investment spending*, our transfers variable is (1) *formula-based and* (2) *between the government and its citizens*, which is crucial to the research hypothesis we have formulated[114]. Moreover, similar to the federal budget balance, our net transfers variable may be considered a *balance* of revenues and expenditures. Ceteris paribus those net transfers may be assumed to be –negatively- correlated with the overall federal budget balance: the more transfers citizens receive, the more negative the federal budget balance will be (ceteris paribus). Hence our net transfers variable may be considered as –however imperfectly- measuring a district's inhabitants' average tangible contribution to the federal budget balance[115]. Unsurprisingly, over our sample period our transfers variable has been *negative* for every individual district and year, as it comprises the 2 major federal revenue categories, and only 1 major federal expenditure category[116].

However, graph 1 shows that transfers per capita are on average more negative for the (22) Flemish districts than for the (21) Francophone Belgian districts[117], which is evidently mainly due to the on average higher gross income per capita of Flemish districts (17,636 euros, versus 15,128 euros for Francophone Belgium). Transfers (in euros) to only 7 out of 22 Flemish districts (Diksmuide -2,308; Eeklo -3,090; Ieper -2,692; Oostende -2,810; Maaseik -3,040; Tongeren -3,021; Veurne -2,769) are less negative than the average (-3,120 euros per capita). Transfers to only 5 out of 21 Francophone Belgian districts (Arlon -3,696; Brussels -3,195; Namur -3,465; Nivelles -5,081; Waremme -3,868) are more negative than the average. Transfers to only 5 out of 22 Flemish districts (Diksmuide -2,308;

[114] In fact it is hard to think of transfers in cash between the Belgian Federal government and its citizens (as well as of transfers in cash between the Belgian Federal government and other Belgian governments) that are *not* formula-based.

[115] A contribution that of course *varies between districts*, as opposed to the federal budget balance itself.

[116] Hence while the transfer concept generally implies an amount *received by* citizens and *provided by* the government, their negative overall sign implies that our transfers variable captures a net transfer *paid to* the government.

[117] We consider all districts located within the Flemish region as Flemish districts and we consider all districts located within the Walloon region as Francophone Belgian districts.

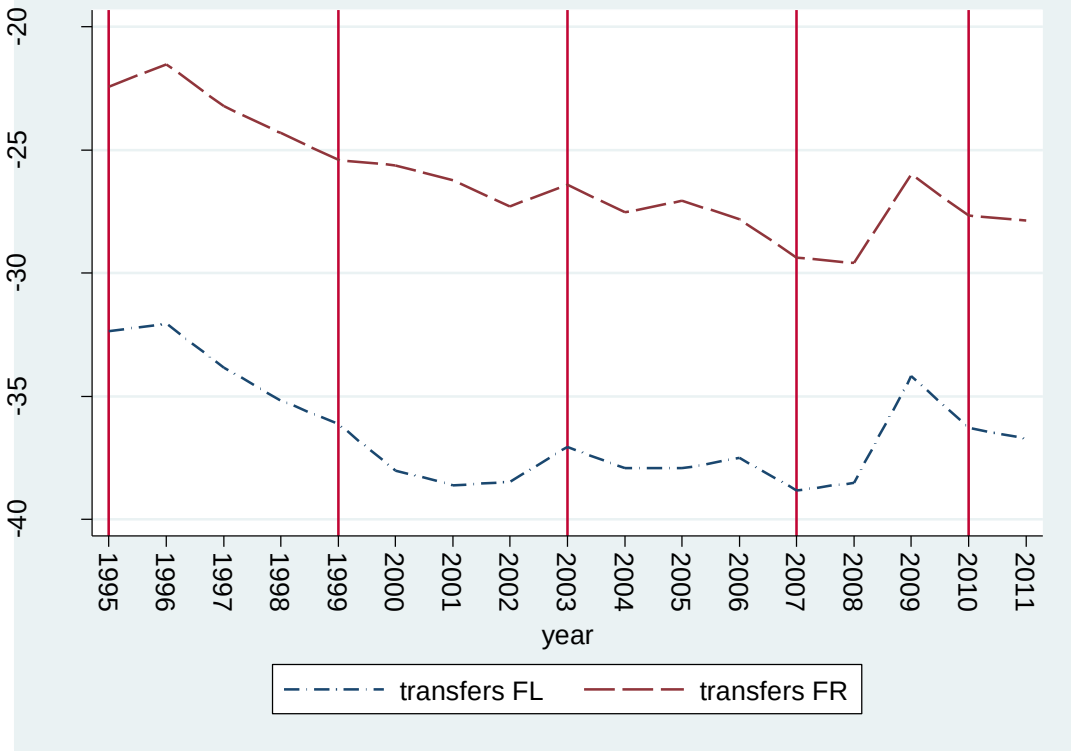
As said above, because voters of the Brussels district overwhelmingly voted for Francophone Belgian parties over our sample period, we also consider the Brussels district as a Francophone Belgian district. Our regression results reported below hardly change if we drop the Brussels district from our sample.

They neither change if we drop the Halle-Vilvoorde district from our sample. The Halle-Vilvoorde district was the only Flemish district in which Francophone Belgian parties obtained parliamentary seats over our sample period, as a non-negligible minority of the inhabitants of this Flemish district surrounding Brussels is Francophone. Voters of the Halle-Vilvoorde district non-negligibly voted for Francophone Belgian parties over our sample period. E.g. in the 2007 federal elections the Flemish parties that won seats in the federal parliament obtained 64.1% of the total vote in the Halle-Vilvoorde district, while the Francophone parties that won seats in the federal parliament obtained 18.5% of the total vote in the Halle-Vilvoorde district.

Finally, our regression results reported below hardly change if we drop the Verviers district from our sample. The Walloon district of Verviers comprises the German speaking Community of Belgium, which also has its separate party system. In 2011, 76,128 of the 282,981 inhabitants of the district of Verviers were German speaking.

Ieper -2,692; Oostende -2,810; Tongeren -3,021; Veurne -2,769) are less negative than the median (Maaseik -3,140 euros per capita). Transfers to only 5 out of 21 Francophone Belgian districts (Arlon -3,696; Brussels -3,195; Namur -3,465; Nivelles -5,081; Waremme -3,868) are more negative than the median.

Graph 1: Transfers per capita (in real 100 euros of 2011)



Note: bars indicate federal election years

Showing separate curves for Francophone Belgian and –Dutch-speaking- Flemish districts in graph 1 makes sense. This is basically because in Belgium no single party that obtained representation in the federal parliament over our sample period obtained a considerable number of votes both in Flanders and in Francophone Belgium[118]. Such split representation may affect individual parties' policy with respect to fiscal transfers, even if they are formula-based[119]. Didier Reynders, a Francophone vice prime minister in the 2010-2014 Federal government -as quoted in Flemish newspapers- explained a more strict income tax treatment of private use of company cars with the fact that "there are more

[118] As said above, exceptions are Flemish parties in the district of Brussels and Francophone Belgian parties in the district of Halle-Vilvoorde.

[119] I.e. the twin facts that Flanders, the region that disproportionately pays for federal deficits on a net basis, is characterised by its own party system, and that also Francophone Belgium, the region that disproportionately "fails to pay" for federal deficits on a net basis, is characterised by its own party system, could increase the effect of the federal deficit on voting behaviour. In other countries that are characterised by a high central government debt and large geographic differences in central tax collection, but that are *not* split geographically according to language, media and/or party system, such as Italy, a district that disproportionately contributes to central government tax revenue may in fiscal matters be expected to rather identify with other districts that disproportionately contribute, irrespective of geographic and cultural distance to that district. I.e. graph 1 for Italy would rather simply compare average transfers per capita to districts disproportionately *paying* for central government deficits with average transfers per capita to districts disproportionately *"failing to pay"* for central government deficits.

company cars in the Flemish province of Antwerp alone than in the entire Walloon region" (De Standaard 2014). A more important example is the finding of "Hoge Raad voor Financiën", a federal fiscal policy watchdog, that the major 2001 income tax cut –while worsening the federal budget balance- mainly benefited inhabitants of the Walloon region (HRF 2010).

A 2nd feature of graph 1 is that for both regions, transfers have become more negative over our entire sample period, with the 2009 economic crisis as the major exception. However, we should not attach too much importance to this feature as this general decrease in transfers could merely be the result of economic growth, which unsurprisingly makes income tax revenue and social security contributions increase in real terms[120]. What is finally noteworthy from graph 1 is that Flemish and Francophone Belgian transfers broadly move *in tandem*, which is an illustration of the formula-based nature of our transfers variable.

In what follows, we will perform a regression analysis of the joint vote share per district of the political parties that are part of the Federal government coalition on transfers per district[121]. As said above, our hypothesis is that the impact of transfers on vote share will be less positive for Flemish districts than for Francophone Belgian districts. This would be because Flemish voters may suspect an increase in transfers to result into a more negative federal budget balance. In turn, this suspicion would be due to Flemish voters being aware of the decades' long weak track record of the Belgian Federal government in terms of budget discipline. The bottom row of table 1 shows that the average federal budget deficit was no less than 7.3% of GDP over the 1970-2002 period. Successive deficits have resulted into a federal public debt of 106.1% of GDP in 2002. In 2011, this ratio still amounted to 96.0%. Ever since the 1980s until the end of our sample period, Belgium has been burdened with one of the highest public debt ratios among OECD countries. An additional assumption on which our

[120] Economic growth may either have decreased or increased social expenditures in cash in real terms over our sample period. On the one hand the need for unemployment benefits decreases if economic growth is stronger and on the other hand Federal governments may have increased social transfers not only with inflation but also with economic growth over our sample period. Because, as said above, social transfers constitute the only expenditure category comprised in our net transfers variable, which also comprises 2 revenue categories, the revenue effect may have dominated the expenditure effect.

[121] Of course we prefer to benefit from the fact that we have transfers data available down to the level of the 43 districts, as e.g. merely analysing votes and transfers at the aggregate levels of Francophone Belgium and Flanders would provide us with far too little variation with a view to deriving conclusions as to impact of transfers on votes.

This goes with the disadvantage that -since Belgian districts are a mere product of history- they vary enormously in terms of population. Mean population of our 43 districts is 242,982 –with a standard deviation of no less than 221,059-, while the largest district –Brussels- on average counted 1,007,531 inhabitants over our sample period and the smallest district –Bastogne- on average counted 42,122 inhabitants over our sample period. Hence, our regression results below could be driven disproportionately by a number of small districts.

When we consider instead the administrative level right above the district level as the unit of observation, i.e. the provincial level, regression results below turn insignificant once adding the interaction of our legislature dummies with the Flanders dummy. (see below) This should not entirely surprise as the shift from the district to the provincial level makes our number of observations decrease from 215 to a mere 55, rendering moreover an IV regression analysis impossible.

Also when we do not weight our right hand side variables for population –effectively giving more weight to the largest districts-, regression results below turn insignificant once we add the interaction of our legislature dummies with the Flanders dummy.

However, when we leave out those districts with an average population below 100,000, regression results are very similar to regression results over the full sample, including those after adding the interaction of our legislature dummies with the Flanders dummy. No less than 13 districts are characterised by an average population below 100,000, of which 4 Flemish (Diksmuide, Eeklo, Tielt, and Veurne) and 9 Francophone (Arlon, Ath, Bastogne, Marche, Mouscron, Neufchâteau, Philippeville, Virton, and Waremme). Leaving them out reduces our number of observations from 215 to 150.

hypothesis is based is that Flemish voters are aware that the interest payments to service a hence increased federal debt disproportionately weight on Flemish taxpayers[122].

Last but not least, this reasoning assumes that Flemish voters are aware of the *formula-based* nature of our transfers category. By law the incidence of the 3 components of our (net) transfers variable largely[123] depends on ability to pay (income tax and social security contributions) resp. on needs (social transfers). Therefore, Flemish voters may suspect an increase in transfers per capita to *their* district to go hand in hand with a -possibly even larger- increase in transfers per capita to the average *Francophone* district[124], as the latter is needier and less able to pay than the average Flemish district[125]. Moreover, Flemish voters may be aware of the "entitlement" nature of our transfers variable, i.e. they are not only formula-based, but –unlike say investment expenditures- increases in our transfers considered in year t tend to be followed by similar increases compared to year t-1 during years t+1 and following, i.e. they tend to have fiscal consequences *for many years to come*.

As a result of all the above, voters of a Flemish district may fear the interest burden consequences of their –current and future- share of transfers received, but they may fear even more the interest burden consequences of the Francophone –current and future- share[126]. Therefore Flemish voters may punish[127] the Flemish parties that are part of the Federal government at the next elections, for presumably having "given in" to the Francophone Belgian government parties' request to increase –formula-based- transfers[128]. We are aware that our research hypothesis spelled out above is rather ambitious in comparison to the existing literature that hypothesises a positive effect of *gross* transfers on government popularity (see below). Our hypothesis assumes a more informed and forward looking voter than the existing literature, which in contrast implicitly assumes that the average voter mainly cares about *gross* transfers to his district[129]. In what follows, we will find weak evidence of an effect

[122] See e.g. Jennes (2014b) for a calculation of the shares of the Belgian regions in federal tax revenue. We assume that Flemish voters assume this gap to persist in the future, when interest payments on the newly accumulated public debt will come due.
Compared to Francophone Belgian voters, Flemish voters may also more strongly pay heed to the possible damage of a large federal public debt to the Belgian *macro-economy*, as the private sector makes up a larger part of the Flemish economy than of the Francophone Belgian economy.

[123] Jennes and Persyn (2015) demonstrate that the transfers category we use as our independent variable of interest is to some extent discretionarily determined however.

[124] For the average Flemish voter, the size of transfers to *his/her own* district seems to be the only plausible indicator of transfers to *Francophone Belgian* districts available, as the average voter arguably does not have any knowledge of statistical sources such as the "Regional accounts", which besides is one of the only publicly available sources of geographically disaggregated data on public expenditures and revenues in Belgium.

[125] In contrast, it is the essence of the *discretionary* transfers investigated in most of the empirical distributive politics literature, that more transfers to one geographic area do *not* lead to more transfers to another geographic area, even on the contrary. The effect of discretionary transfers on government popularity could hence be rather different from the effect of formula-based transfers on government popularity.

[126] Alternatively stated, a Flemish district may fear the interest burden consequences of the share of transfers received by districts that are *poorer* than their own one *in general*, but these poorer districts are overwhelmingly concentrated in Francophone Belgium. (see also graph 1)

[127] Again, relatively speaking, i.e. compared to Francophone Belgian voters.

[128] Alternatively to taking the point of view of Flemish voters, our research hypothesis could as well have been spelt out taking the point of view of Francophone Belgian voters. Of course such an alternative hypothesis would not have changed the interpretation of our regression results below.

[129] Our hypothesis reminds of the well-known hypothesis of Barro (1974) that consumers save in anticipation of a tax increase to follow the build-up of government debt. This so-called Ricardian equivalence theorem however has not been fully validated empirically. In contrast, Lyytikäinen and Tukiainen (2013) find the rational voter hypothesis confirmed in Finland, in the sense that Finnish voters base their decision to go and cast their vote on its expected impact on the election outcome.

of transfers on votes in Flanders that is significantly smaller than the effect of transfers on votes in Francophone Belgium.

In what follows, we will first briefly overview the related literature. Next, we will discuss the data in more detail. Thirdly, we will outline our empirical strategy, before moving on to a presentation and a discussion of the regression results. We will end with a conclusion.

2. Situation of our study within the literature

As said above, to our knowledge (1) the central government's debt game (2) between different regions (3) represented within one and the same central government has never been tested empirically. Moreover, to our knowledge, all theoretical and empirical studies thus far on the political economy of central government debt took debt/deficits as the *dependent* variable. (see e.g. Alesina and Tabellini 1992, Alesina and Perotti 1994, Persson and Tabellini 2000, Roubini and Sachs 1989, de Haan and Sturm 1997, Volkerink and de Haan 2001, and Huber e.a. 2003)

In contrast, we will include a debt/deficit component into our *independent* variable of interest. (see below) As stated already above, we will test if Belgian regions have different incentives to make the central government run a deficit by empirically verifying if net fiscal transfers are appreciated differently by voters from the 2 major Belgian language communities. These language communities are also different *political* communities, each represented by a *different* set of political parties within the Federal government and each effectively wielding veto power over Federal government policy. We will verify differing voters' appreciation by regressing the vote share of the governing coalition on previous transfers to Belgian districts. Therefore, our investigation is rather related to the extensive empirical political economics literature on "pork barrel", investigating if distributive politics practised by the incumbent party (parties) buys votes during the next elections. However, to our knowledge this "pork barrel" literature does not at all pay attention to possible geographic *differences* in popularity of the *same* kind of transfers[130].

Veiga and Veiga (2013) find that election year increases in transfers by the central government to Portuguese municipalities secure additional votes in favour of the party in power at the central level. Stratmann (2013) demonstrates that earmarks voted by US Congress in favour of particular electoral districts buy more votes in these districts. A more recent literature makes use of a Regression Discontinuity Design (RDD) –which identifies the effect of transfers on votes by focussing on electoral districts in which an election was barely won or barely lost- to assess if extra transfers buy more votes. This RDD literature first investigates if having gained power leads to more transfers to districts that have voted in favour of the incumbent, and it next investigates if the incumbent wins extra votes during the next elections thanks to these extra transfers. Brollo and Nannicini (2012) for Brazil, Bracco e.a. (2012) for Italy, Curto-Grau e.a. (2012) for Spain, and Migueis (2012) for Portugal all find that extra transfers buy extra votes in favour of the incumbent party (parties) during the next elections, either at the central, at the local or at both levels.

[130] At least as vast is the related literature on the reverse effect, i.e. investigating if incumbent governments steer extra transfers to particular districts precisely with a view to obtaining extra votes, as well as the related literature investigating if *incumbency as such* buys extra votes, i.e. investigating if being in power leads to remaining in power. For examples of the former literature, see Dahlberg and Johansson (2002) for Sweden, Knight (2008) for the US, and Jennes and Persyn (2015) for Belgium. For examples of the latter literature, see Lee (2001) and Ansolabehere and Snyder (2002) on the US.

What makes our study stand out from the "pork barrel" literature is (1) that our transfers variable is a *net* transfers variable, (2) that we will use it as an indicator to citizens for *debt accumulation*, since an increase in net transfers to citizens ceteris paribus means an increase in debt, and (3) that we would expect the Belgian region that de facto pays most of the debt service burden, i.e. Flanders, to reward the incumbent parties *to a lesser extent* with extra votes –i.e. to punish them relatively speaking- for steering transfers to this region.

3. Data

As said already, graph 1 above summarises the evolution of our *independent* variable of interest over time and between Flanders and Francophone Belgium (in real 100 euros per capita). The evolution is remarkably parallel for Flanders and Francophone Belgium, and the difference in transfers per capita between Flanders and Francophone Belgium remains relatively stable over our sample period. However, the standard deviation of transfers to Flemish districts (880 euros per capita) is smaller than the standard deviation of transfers to Francophone Belgian districts (1016 euros per capita), despite the average transfers amount of Flemish districts (-3645 euros per capita) being considerably more negative than the average transfers amount for Francophone Belgian districts (-2619 euros per capita). Vertical bars mark federal election years in graphs 1 and following. The 1st full legislature of our sample period (1995-99) is characterised both for Flanders and Francophone Belgium by an overall steep decrease in transfers received. This is because the then Federal government had embarked upon a program of strong budget deficit reduction with a view to being allowed into the Euro currency zone. Another remarkable legislature is the 2007-10 legislature[131]: the sudden outbreak of the 2009 economic crisis resulted into an equally sudden surge in net fiscal transfers received.

Graph 2 shows the evolution of our *dependent* variable "difference in government vote share" over time and between Flemish and Francophone Belgian districts[132]. However, we cannot simply calculate our dependent variable as the difference between total votes obtained per district by the incumbent Federal government coalition and total votes obtained per district by its predecessor. This would be like comparing apples and oranges, as most federal elections over our sample period resulted into a *change in the composition* of the Federal government coalition[133]. Therefore we have constructed our

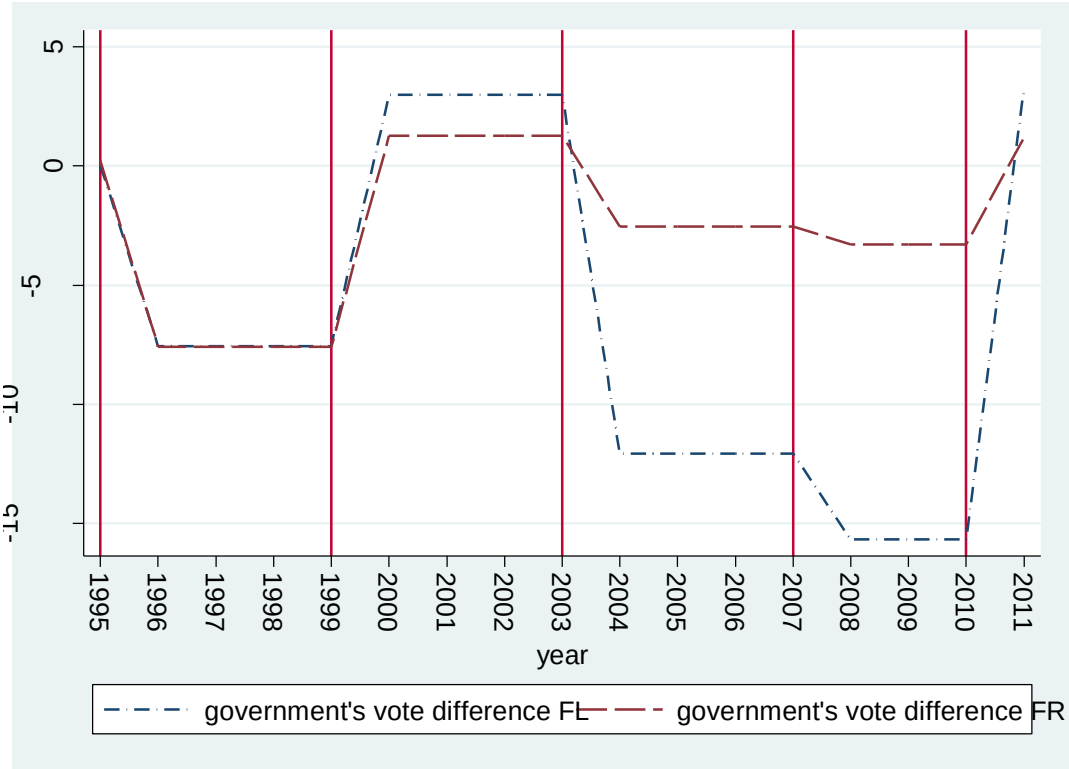
[131] This legislature was the only one over our sample period that did not last its full 4 years, as the then government was brought down in 2010 by an institutional conflict between Flemish and Francophone Belgian coalition parties.

[132] Data on votes were kindly provided by Jo Buelens and Kris Deschouwer (VUB). Additional data on votes was obtained from the website of the Federal Ministry of the Interior: http://www.ibzdgip.fgov.be/result/nl/main.html
In contrast to Belgian practice of allocating parliamentary seats to parties, we have calculated vote shares as the ratio of votes obtained by a particular party to *total eligible voters*, i.e. including voters who cast non-valid votes, who cast "blank" votes, and who did not cast their vote (despite voting being mandatory in Belgium). The Belgian real world allocation of parliamentary seats disregards non-valid votes, "blank" votes, and votes not cast, although these votes may well be an expression of voters' disapproval of the governing parties. We calculate vote shares somewhat differently from the Belgian real world allocation of parliamentary seats because we will use non-valid votes, "blank" votes, and votes not cast to construct an alternative dependent variable later on. Our different calculation decreases the size of our dependent variable somewhat, both in levels and in differences, compared to standard Belgian practice, as non-valid votes, "blank" votes, and votes not cast taken together on average amounted to 20.4% of the total eligible vote over our sample period (17.5% for Flanders and 23.5% for Francophone Belgium).

[133] Such a change implied a change in the identity of the governing parties, a change in their number, or a change in both.

dependent variable as the difference between the joint vote share (per district) obtained by the parties that were part of the governing coalition during federal elections t and the joint vote share (per district) obtained by *those same parties* during federal elections t-1, *when one or more of these parties may not have been part of the outgoing governing coalition*. Our dependent variable hence measures the joint increase/decrease in popularity of governing parties after having governed during one federal legislature[134]. As our dependent variable is hence expressed as a difference[135], we will also express our independent variables in differences (and not in levels as shown in graph 1).

Graph 2: Difference in vote share (%points) obtained by the governing coalition between 2 federal elections



Note: bars indicate federal election years

Source: Jo Buelens and Kris Deschouwer (VUB); http://www.ibzdgip.fgov.be/result/nl/main.html (Federal Ministry of the Interior); own calculations.

We will test in our regression analysis below if transfers in legislature t affect the result of the election at the end of *that same legislature t.* I.e. we will only consider mean transfers *per legislature*, as by definition elections are only held once per legislature. Therefore, we have allocated in graph 2 the average vote share difference for Flemish resp. Francophone Belgian districts between 2 federal elections to *every single year* of the legislature concerned. Such "retroactive" allocation of vote share differences should reflect the possibility that transfers occurring *in any year* of a particular legislature

[134] We of course cannot and should not take into account the –possibly non-negligible- effect of a governing coalition governing for a 2nd legislature in a row –like e.g. during the 2003-07 legislature- on government popularity (see also Stratmann 2013), as there is obviously no variation over our 43 districts with respect to time in office of the Federal government coalition.

[135] For Flanders, graph 2 shows the average vote difference for all 22 Flemish districts. For Francophone Belgium, graph 2 shows the average vote difference for all 21 Francophone Belgian districts.

may affect voting behaviour *at the end* of that legislature. However, our presentation of a particular vote share difference in graph 2 as a horizontal line spanning an entire legislature comes with the cost of possibly giving the wrong impression that government parties already knew their election result before the end of the legislature.

Graph 2 shows that most incumbent Federal governments over our sample period see their vote share *decrease* at the end of the legislature during which they governed, as both the curves for Flanders and for Francophone Belgium are below the 0% line for most years of our sample period[136]. E.g. during the 1999 elections the parties governing between 1995 and 1999 saw their joint vote share decrease by about 7.5%points[137]. In contrast, the 1999-2003 government as well as the 2010-2014 government saw their vote shares *increase,* although by considerably less percentage points than the 7.5%points decrease suffered by the 1995-1999 government[138].

Most remarkable from graph 2 is the divergence in vote difference between Flanders and Francophone Belgium over our sample period. More in particular, the 2 legislatures between 2003 and 2010 are characterised by a large loss of vote share for Flemish governing parties, while the Francophone Belgian vote losses were much smaller. As a result, the total average as well as the total standard deviation of the Flemish government parties' votes difference (-5.26 resp. 7.35%points) are much larger than the total average and the total standard deviation of the Francophone Belgian government parties' votes difference (-1.96 resp. 3.94%points)[139]. Participation to a Belgian Federal government seems much more risky –in terms of vote share- for Flemish parties than for Francophone Belgian parties. Also, the Flemish evolution from a loss of vote share to a gain in vote share between the 2007-10 and the 2010-14 legislatures is less remarkable than it seems from graph 2. This is because 2 of the 3 Flemish parties governing between 2010 and 2014 already governed during the 2007-10 period and saw their vote share –slightly- increase in 2014 *from a historically very low starting point* in 2010[140].

As shown by graph 2, our sample period only comprises 5 federal elections and 6 legislatures, which will somewhat constrain our empirical analysis below. As also shown by graph 2, election years themselves are assigned vote differences concerning the *previous* legislature, although elections always took place as early as in May or June over our sample period. Such assignment takes into account the possibility of a political business cycle, according to which governing political parties would concentrate transfers right before a federal election to try and increase their vote share during that election.

---

[136] Again, showing separate curves for Flemish and Francophone Belgian districts in graph 2 makes sense as in Belgium no single party that obtained representation in the federal parliament over our sample period obtained a considerable number of votes both in Flanders and in Francophone Belgium.

[137] The mostly negative average vote differences stand somewhat in contrast to the literature on the *positive* effect of incumbency on vote share and on being re-elected into office. (see e.g. for the US Lee (2001) and Ansolabehere and Snyder (2002))

[138] Only one year of the 1991-1995 legislature and only one year of the 2010-14 legislature have been included into our sample, as we were only able to collect transfers data from 1995 onwards and until and including the year 2011.

[139] These "different differences" may partly explain why in a number of cases we will find a statistically significant association between difference in transfers and government parties' vote difference for Flanders, but not for Francophone Belgium. (see below)

[140] With respect to the 1995 federal elections, the vote share difference with the previous elections –the 1991 elections- was of course not zero, in contrast to what graph 2 suggests. Graph 2 shows a zero for the 1991-1995 legislature, simply because we do not consider the 1995 vote share difference, in turn because we only have transfers data at our availability from 1995 onwards. Because our regression analysis will be in first differences (see below), it will drop the year 1995.

4. Empirical strategy: first differences (FD) and instrumental variables (IV)

Our regression equation estimated in the regression tables below can be summarised as follows:

$$D.government's\ vote\ share_{it}$$

$$= \alpha_1\ D.(net)\ transfers\ per\ capita_{it} + \alpha_2\ D.(net)\ transfers\ per\ capita_{it} * FL$$

$$+ \alpha_3 D.X_{it} + \alpha_t + \varepsilon_{it}$$

With FL a dummy variable taking the value of 1 in case of a Flemish district, with X_{it} a set of demographic, economic and political control variables, and with α_t a legislature fixed effect.

The legislature fixed effect α_t controls for unobserved legislature-specific shocks that may be correlated both with transfers and government popularity[141]. It ensures i.a. that our coefficients of interest α_1 and α_2 measure the association between transfers and government popularity after having filtered out *country-wide but legislature-specific* shocks. An example of such shocks are "tide" effects in party popularity, such as a country-wide surge in the popularity of a protest party[142], or a particular political issue made salient by the media during a particular pre-election period.

The combination of the first difference (FD) transformator D. with the use of legislature dummies in our above regression equation makes that identification of the association between the government's vote share and transfers is only based on changes in both variables that *simultaneously* differ over time *and* between districts. As our variables are expressed in differences, district fixed effects are eliminated[143]. The FL dummy itself is not included as a separate regressor, as its value does not change over time. It would therefore also be eliminated by the FD transformation.

[141] In this respect, Flemish media hold the view that "every Belgian election holds its surprise". An example was the 2010 federal election, which was a "snap election". I.e. while the federal parliament is elected for 4 years, the 2010 election took place one year earlier already because of the fall of the Federal government in 2010.

[142] Or a sudden country-wide surge in the popularity of a *non*-protest party. See e.g. Allers and Merkus (2013) for the "tide effect" of national party preferences on municipal election results in the Netherlands.
Legislature fixed effects should also take care of possible manipulation of election dates by politicians, e.g. to try and take advantage of favourable popularity polls, as such manipulation may be assumed to have the same impact on all Belgian districts. Manipulation of election dates is a problem raised by e.g. Pettersson-Lidbom (2006 p. 11). Over our sample period, only the federal elections of 1995 and of 2010 were held considerably earlier –i.e. about 6 months resp. about 1 year earlier- than their due date –i.e. earlier than after 4 years after the previous elections.
Additionally including a *trend* variable –next to a set of *legislature dummies*- does not appear to affect our regression results. In contrast to legislature dummies, a legislature trend variable assumes the passing of time to be *smoothly* related to our dependent variable. This seems rather less probable given the *political* nature of our dependent variable, and given the fact that our dependent variable is expressed in differences. Including a trend variable –though "asking less" from our limited data set- would thus be less general than including a set of legislature dummies.

[143] Moreover, because of the *need* to express our dependent variable as the *difference* between the joint vote share of governing parties during the next election and their joint vote share during the previous elections, a fixed effects within regression strategy would not be feasible.

Additionally we cluster our standard errors at the district level, because error terms may be correlated within districts over time[144].

In a next step, we will aim at making possible a causal interpretation of the association between the government's vote share and transfers by using instrumental variables (IV) estimation[145].

Results of our empirical strategy are shown in a stepwise way in the tables below.

5. Results of the regression analysis

5.1. First differences (FD) regression results

5.1.1. Regression results *without* legislature*FL dummies added as extra controls

Table 2 shows the results of a simple FD regression of the government's vote share on transfers. All variables are *means* taken per district and legislature[146]. For this reason, we remain with only 215

[144] However, error terms may alternatively be correlated *across districts within legislatures*. But when we cluster our standard errors at the legislature level instead of at the district level, standard errors reported below together with our successive regression results, as well as the significance of our coefficients of interest, hardly change.
In contrast, when clustering standard errors at the region level (Flanders and Francophone Belgium) instead of at the district levels, the below reported standard errors are unsurprisingly considerably smaller and the below reported coefficients are considerable more significant (and even very significant in many specifications). This implies that we should cluster standard errors at the district level instead of at the region level.

[145] A method to isolate random changes in our independent variable of interest that would be preferable to IV estimation would be a Regression Discontinuity Design (RDD), but we failed to identify discontinuities in transfers between districts over our sample period.

[146] We opted not to exploit the fact that we have *annual* transfers data at our disposal, because federal elections of course are not held annually.
Because we have at our disposal only one year of transfers data concerning the 1991-1995 legislature –i.e. 1995 transfers- and only one year of transfers data of the 2010-14 legislature –i.e. 2011 transfers-, our regression results could be biased in case transfers in these 2 years would not be representative for average transfers over the *entire* legislature concerned. 1995 may be unrepresentative because it is an election year. 2011 may be unrepresentative as in 2011 a caretaker Federal government –in the shape of the outgoing Federal government- was in place due to the most protracted government negotiations in Belgian history then taking place (from June 2010 until December 2011).
However, regression results below are similar when alternatingly dropping the years 1995 and 2011 from our sample period, despite the considerable loss of observations –one entire legislature- involved and except for the regression specifications including the lagged dependent variable (LDV). (see below) The latter is understandable, as simultaneously including the LDV and dropping either the 1991-1995 legislature or the 2010-14 legislature reduces the number of observations to only 86 (= 2*43).
Another way in which we verified the sensitivity of our choice to consider simple *means* of transfers per legislature, is by explicitly taking into account the possibility of a political business cycle. To this effect, we calculated mean transfers per legislature by only taking the mean of transfers during the *final 2 calendar years* of each legislature. As said above, federal elections always took place in May or June over our sample period. The exception to our way to calculate transfers when accounting for a political business cycle is the 2007-2010 legislature. Because the 2010 elections were *unexpected*, as they were the consequence of the fall of the Federal government, and as only 3 years of the legislature concerned had elapsed at that time, we "calculated" mean transfers over the 2007-2010 legislature as transfers in the *year 2010 only*.

observations (= 5 legislatures * 43 districts), out of an original total of 731 observations (= 17 years * 43 districts). 1 legislature –the 1991-1995 one- is lost because of the FD transformation.

Column (1) –at first sight surprisingly- shows a significantly *negative* association between transfers and votes. In column (2) we additionally interact the transfers variable with a dummy taking the value of 1 for Flemish districts. The resulting interaction variable is our actual major independent variable of interest as it measures the *difference* in association between transfers received and governing coalition's vote share between the average Francophone Belgian and the average Flemish district. The coefficients in column (2) are to be interpreted as follows: while there does not appear any association between transfers received and vote share obtained in Francophone Belgium, Flemish coalition parties' joint vote share significantly *decreases* with an *increase* in per capita transfers.

Table 2: FD regression of the government's vote share on transfers

	(1) D.govt vote	(2) D.govt vote	(3) D.govt vote	(4) D.govt vote
D.over 60			-4.090***	-4.324***
			(1.208)	(1.056)
D.unemployed			0.754	2.021**
			(0.736)	(0.768)
D.under 18			-4.312**	-3.408*
			(1.817)	(1.811)
D.gross income			-0.0232	-0.157
			(0.175)	(0.137)
D.ministers			-0.218	-0.517
			(0.667)	(0.509)
D.transfers	**-0.417****	**-0.181**	**-0.258**	**-0.00492**
	(0.187)	**(0.163)**	**(0.273)**	**(0.155)**
D.transfers*FL		**-0.880*****		**-1.565*****
		(0 .248)		**(0.298)**
N	215	215	215	215

Robust standard errors clustered at the district level in parentheses; legislature dummies included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

However, column (3) shows that the association found in column (1) becomes insignificant once a number of demographic and economic controls that may both affect transfers and government popularity are included: the shares of over 60 years' old, under 18 years' old, and unemployed, as well as gross income per capita. The shares of over 60 years' old and of under 18 years' old are meant to capture the possibly differing popularity of governing parties across age groups, age groups that are also treated differently in terms of transfers. In contrast, unemployment share and gross income per capita are meant to capture i.a. the possibly geographically heterogeneous impact of the economic

Regression results are broadly similar when explicitly taking into account the possibility of a political business cycle, suggesting that such a cycle was indeed at play over our sample period. However, as 2011 is the only year of the 2010-2014 legislature for which we have transfers data, we need to drop that legislature from our observations with a view to checking for a political business cycle. The latter reduces our –from the start already limited- number of observations even more. An additional disadvantage of the strategy taking into account the possible occurence of a political business cycle is that by dropping 2011 we are left with observations for one of our IVs -i.e. crisis-related temporary unemployment benefits- concerning only 1 legislature (the 2007-2010 legislature). This seems to undermine the validity of our "crisis-related temporary unemployment benefits" instrument within the context of an IV approach (see below).

cycle on the government's popularity: transfers may –indirectly- affect government popularity *negatively*, through an increase in the number of unemployed or –more generally- through a decrease in gross income, both of which may be distributed unequally over Belgian districts[147].

Additionally, a *political* control that may both affect transfers and government popularity is included, measuring the number of federal ministers –weighted by a district's population share- in whose electoral district a particular district is situated. Indeed, Jennes and Persyn (2015) show that a Belgian federal minister manages to increase those same formula-based net transfers that we use as the independent variable of interest, to his/her electoral district[148].

A priori, the sign of these controls could be positive or negative[149], except for the ministers variable, which is expected to show a positive coefficient. Surprisingly the minister variable however appears to be negatively associated with government popularity, though this association is statistically insignificant[150].

As such, column (3) already presents a remarkable result from the perspective of the existing literature on distributive politics: in Belgium taken as a whole an increase in transfers does *not* seem to be associated with an increase in government popularity, in contrast to the existing literature.

In column (4) we again additionally interact the transfers variable with a dummy taking the value of 1 for Flemish districts. As our transfers variable is expressed in 100 euros per capita, the –once again strongly significant- coefficient on the interacted variable in column (4) is to be interpreted as follows: while there does not appear any association between transfers received and vote share obtained in Francophone Belgium, Flemish coalition parties' joint vote share *decreases* with no less than 1.6%point[151] for every *increase* of per capita transfers amounting to 100 euros. As the within standard

[147] By including the shares of unemployed, over 60 years' old and under 18 years' old as control variables, pure changes in demography and in the economic cycle are filtered out of the transfers variable. Therefore from column (3) onwards the transfers variable only measures changes in the individual benefit *amounts* themselves received by the unemployed, the over 60 years' old and (the parents of) the under 18 years' old, rather than the changes in the *number* of unemployment benefits, retirement benefits and child allowances.

[148] Our inclusion of the number of federal ministers as a political control is inspired on Stratmann (2013), who controls for the presence of a senior member of Congress in a district when regressing re-election on "pork barrel" spending. An alternative for this political variable may have been the number of federal members of parliament weighted by population. However, the executive is considered more powerful in Belgium than the legislative.

[149] At first sight it is surprising that the coefficient on the share of unemployed is positive in column (3), and even significantly positive in column (4). It could be interpreted as unemployed who do not blame the government for their unemployment, but instead vote for one of the government parties in the hope of them increasing unemployment benefits, as over our entire sample period, Federal governments consisted of at least 1 left or centre left party per language group. Alternatively, the positive sign could be interpreted as a relative disregard of the economy –with an increase in unemployment as a consequence- by a Federal government once this government becomes more popular.

The –sometimes significantly- positive sign of the unemployment control persists in most of our other regression results reported below. However, it appears to hardly affect our coefficients of interest, as the latter hardly change whenever we omit the unemployment control from each of our regression specifications reported in this paper.

At first sight it is also surprising that the coefficient on gross income per capita is negative in table 2 (although insignificantly negative). However, this may be due to multicollinearity with the variable measuring the share of unemployed. The correlation between our (differenced) share of unemployed variable and our (differenced) gross income variable is strong (-0.4).

[150] Again, this result runs counter to the rather extensive literature on the positive effect of incumbency on vote share and on being re-elected to office. (see e.g. for the US Lee (2001) and Ansolabehere and Snyder (2002))

[151] This decrease would be somewhat larger if we had not measured the government's vote share in relation to total *eligible* votes but to total votes *validly cast*.

deviation of transfers per Flemish district over our 6 legislatures is 239 euros per capita, the typical variation in government vote share due to variations in transfers –expressed in 100 euros per capita- over time equals about -3.7%points (i.e. -1.6%point times 2.39), with a p value of 0.000.

This result is surprising, as we had expected -on the basis of table 1 above and on the basis of the abovementioned distributive politics literature on the positive effects of transfers on incumbency- a negative coefficient for the interacted transfers variable to go hand in hand with a significantly *positive* non-interacted transfers coefficient. A statistically significant interacted transfers coefficient would then have provided tentative support to our hypothesis. Otherwise stated, it is not so much the significantly negative interaction coefficient that surprises[152], but rather the insignificant and small non-interacted coefficient.

One could argue that it would be the fact that a district's transfers are *below the mean* (or below the median) that is associated with a lower share of the votes of government parties, rather than the fact that a district is *situated in Flanders*. I.e. in those few Francophone Belgian districts that are characterised by transfers that are below the mean (or below the median), the association between transfers and the government's vote share could be as small as in Flanders. However, when we replace the FL dummy with a dummy "below mean" (or with a dummy "below median") taking the value of 1 in all districts –Flemish or Francophone Belgian- that are characterised by transfers that are below the mean (or below the median), the association becomes smaller and less significant. The coefficient concerned is only -0.548, with a p value of 0.12. (When replacing the FL dummy with the "below median" dummy: a coefficient of only -0.811 and a p value of 0.02). The association becomes even insignificant when we simply replace the FL dummy with the continuous variable gross income, based on the hypothesis that extra transfers would in particular be associated with less votes for the government in cases in which also gross income increases.

The fact that in no Flemish district voters are able to vote for (or against) Francophone Belgian government parties, and vice versa[153], as well as the fact that Flanders and Francophone Belgium hardly share any geography, language or media, constitute additional arguments to distinguish between Flemish and Francophone Belgian districts[154]. This is preferable to distinguishing between *relatively speaking* "transfers receiving" and "transfers providing" districts, or between districts that see their gross income decrease and districts that see their gross income increase. Spolaore (2008) demonstrates theoretically that richer regions are reluctant to redistribute income to poorer regions within the same country, *in particular* if these regions are culturally heterogeneous.

We have also re-constructed our independent variable of interest so that it becomes a better measure of the Federal government's budget deficit. To this effect, we have weighted the transfers variable

[152] As the correlation between our (differenced) transfers variable of interest and (differenced) gross income is strong (-0.5), one may at first sight worry about multicollinearity between transfers and gross income. However, as the coefficient on our variable of interest is significant, such a worry does not seem justified. Moreover, the coefficient of the interacted variable remains as significantly negative and of the same size when omitting gross income as a control.

[153] As stated above: over our sample period with the exceptions of Brussels and Halle-Vilvoorde.

[154] Political parties being split along linguistic-geographic lines is not even a necessary condition for focusing our regression analysis on the Flemish / Francophone Belgian distinction: also if parties were *not* split along linguistic-geographic lines, finding a different association between transfers and Federal government popularity in Flanders and in Francophone Belgium would be interesting. However, political parties being split along linguistic-geographic lines adds an extra dimension, as such a split increases the likelihood of differing associations between transfers and Federal government popularity in Flanders and in Francophone Belgium having differing impacts on individual parties' *policies* with respect to transfers.

with the federal budget balance, assuming that voters may react more favourably to transfers received in years in which the budget deficit is low compared to years in which the budget deficit is higher. Again, this may be the case because voters –in particular Flemish voters- may fear a deficit increase to result into an increased interest bill, to be financed with higher taxes in the future. Moreover, over our *entire* sample period, the federal budget balance was to some extent a political issue, also given the chronically large federal budget deficits of the past decades and the large federal debt that has resulted from these. By adding an extra interaction with the federal deficit to our variable of interest, more importance is attached to transfers in years with a worse budget balance.

However, as a result (not shown), the size of our coefficient of interest decreases compared to column (4), while we had expected it to increase[155]. Perhaps this result means that citizens are less aware of the *overall* federal fiscal situation than of transfers to their district *in particular*. Indeed, transfers show up "inside the wallet" of voters in the same year as the year to which they are allocated in the "Regional accounts" statistics of the central bank. In contrast, the federal fiscal balance becomes known to voters –sometimes much- later –if at all- than during the year to which it is "allocated" in the "Government accounts" statistics of the central bank[156]. Or perhaps this result means that Flemish voters would be averse to an increase in net fiscal transfers *as such*, i.e. averse to redistribution towards Francophone Belgium *even when the budget would be balanced*, rather than averse to redistribution in the shape of future interest payments. In particular, Flemish voters may fear that such a redistribution would –either currently or in the future- be funded with less federal expenditures in Flanders or more taxes raised out of Flanders that are *not* included in our independent formula-based net transfers variable (such as federal investment expenditures or VAT revenue). If the latter held, our regression analysis would be about measuring the effect of net fiscal transfers *as such* on government popularity, rather than on the effect of federal budget deficits on government popularity.

5.1.2. Regression results with legislature*FL dummies added as extra controls

However, the regression results reported in table 2 change considerably once we add as extra controls *the interaction of the legislature dummies with the FL dummy*[157]. Hence next to interacting our variable of interest with the FL dummy, we also interact our legislature dummies with the FL dummy. Although such an addition seems demanding[158], it also seems warranted, because the unobserved legislature-specific shocks for which the legislature fixed effects control may well *differ* between Flanders and Francophone Belgium. This is because –at least over our sample period- these 2 language communities were not only lacking a common geography and language, but also common media and political parties, and because the constitution guarantees both language communities a

[155] The reduced size of the association between government popularity and transfers when weighting transfers for the size of the deficit remains when lagging the deficit variable with 1 period. A lag seems justified as voters of course are aware of the size of the deficit of a particular fiscal year at best during the next year.

[156] A notorious case in point is the sudden and sizeable budget deficit of 2005. The then government had to revise its original accounting for the 2005 budget balance considerably downwardly after the European Commission obliged the government to include its takeover of debt from the public railways in 2005 into the government budget. The government disputed this decision of the European Commission in court, but –as it appeared years later- to no avail.

[157] See Levitt (1997) for the only previous study we are aware of that includes region-time dummies into a regression specification where the actual unit of observation is below the regional level.

[158] As the addition of the interaction of the legislature dummies with the FL dummy –i.e. of 5 extra control variables- increases the risk of saturating our model, given that we only have 215 data points at our disposal.

fixed share of cabinet ministers regardless of the election result. These features lead some observers to talk about "2 separate democracies in one country" and about "2 separate elections taking place on federal election day".

In Belgium, a legislature-specific and transfer-related shake-up of the party landscape[159] due to the surge of a protest party in the opinion polls or due to an outbreak of a political scandal in the run-up to a particular federal election[160] in *one* of the 2 language communities may hardly cause ripples in the *other* language community. An example –though not clearly related to the formula-based transfers under study- was the diverging behaviour of both Green parties by the end of the single legislature -1999-2003- in which they were part of the governing coalition. The leading minister of the Flemish Green party resigned (and was replaced by another member of the Flemish Green party) in August 2002 after the government had approved an arms export deal with Nepal, while no minister of the Francophone Green party resigned. In contrast, all ministers of the Francophone Greens resigned in May 2003 after the government had approved new flight routes over the -mainly Francophone- city of Brussels for airplanes departing from or bound for Belgium's main airport, while no minister of the Flemish Greens resigned.

Another example of heterogeneity between Flanders and Francophone Belgium at the federal level over our sample period is that the *composition* of the Federal government was "asymmetrical" during the 1999-2003 and 2007-2010 legislatures. During the former it included the party VU at the Flemish side, and therefore one more party (4) at the Flemish side than at the Francophone Belgian side (3); during the latter it did not include the Flemish socialists while it did include the Francophone Belgian socialists, resulting in the Federal government consisting of 2 Flemish parties and 3 Francophone Belgian parties.

Another case of heterogeneity between Flanders and Francophone Belgium with respect to composition of the Federal government coalition is that from 1999 onwards the share of the total –valid- Francophone Belgian vote that was represented in the Federal government has been considerably larger than the total –valid- Flemish vote share represented in the Federal government. This was because since 1999 Francophone Belgian governing parties have been far larger (in terms of Francophone Belgian vote share) than Flemish governing parties (in terms of Flemish vote share). During the 2010-2014 legislature the 3 Flemish governing parties were not even supported by half of the –valid- Flemish vote cast[161].

As a final illustration of the potential usefulness of adding an interaction of the legislature dummies with the FL dummy, graph 2 above shows the divergence of changes in government popularity between Flanders and Francophone Belgium over our sample period. In general, graph 2 illustrates the larger size of the shocks to the "party landscape" in Flanders than in Francophone Belgium over our sample period. In particular, one perceived cause of the heavier loss of Flemish government

[159] In the entire Belgian history, party strength has differed between Flanders and Francophone Belgium, with Christian-democrats collecting more votes in Flanders and socialists collecting more votes in Francophone Belgium. In recent decades, an additional major difference has been the far larger fragmentation of the party landscape in Flanders than in Francophone Belgium, mainly due to the varying success of rightwing and/or independentist protest parties in Flanders.

[160] Although it is hard to see an immediate *relationship* between transfers and such legislature-specific events.

[161] The average vote share obtained by the governing coalition at the start of a legislature over our sample period is 44.2%. The average Flemish vote share obtained by the governing coalition is 36.9%, while the average Francophone Belgian vote share obtained by the governing coalition is 51.8%. (Because we measure the average vote share obtained by the governing coalition as *the share in total eligible voters, rather than in total valid votes cast*, it is well possible that this share is below 50%.)

parties in 2007 and 2010 was that they had repeatedly failed to implement measures perceived as leading towards more autonomy of the Flemish region. In contrast, the general Francophone Belgian perception was that Francophone Belgian governing parties had successfully resisted such measures. Flemish voters' resentment of Francophone Belgium and of the Federal government –including of the fiscal transfers implicit in the federal budget from Flanders to Francophone Belgium- has arguably increased considerably over our sample period, as measured by the vote share of Flemish independentist and protest parties[162].

Table 3 shows the regression output. Again, column (3) presents an interesting result as such from the perspective of the existing literature on distributive politics. In contrast to column (3) in table 2 above, we now find that in Belgium taken as a whole an increase in transfers *is* associated with an increase in government popularity. More in particular, coalition parties' joint vote share *increases* with 0.5%point for every *increase* of per capita transfers of 100 euros. This result is more in line with the existing literature, in contrast to our finding in column (3) of table 2 above.

Column (4) in table 3 shows the same sign for the coefficients of our 2 variables of interest as column (4) in table 2: a positive sign for the non-interacted transfers variable and a negative sign for the interacted transfers variable. But the significance switches: now the coefficient of the non-interacted coefficient of interest is significant while the coefficient of the interacted coefficient of interest is insignificant[163]. Also the joint size of our 2 coefficients of interest is considerably smaller when adding the interacted legislature dummies[164]. Overall we now obtain regression results that are more in line with the existing literature: the government's popularity is positively associated with transfers –increasing with 0.5%point of the vote share for every increase in transfers per capita of 100 euros-, *both in Francophone Belgium and in Flanders*[165]. This seems less so in Flanders, but the difference between Flanders and Francophone Belgium is insignificant[166].

[162] While Francophone voters seem rather to show disagreement with the political system by not casting their vote or by casting a blank or an invalid vote.
Independence of Flanders would reduce fiscal transfers from Flanders to Francophone Belgium to zero.

[163] Also, several of the coefficients of the -interacted and non-interacted- legislature dummies –not shown- are significant in table 3, similarly to the high significance of most of the –non-interacted- legislature dummies –not shown- in table 2.

[164] The joint size of our 2 coefficients of interest would be somewhat larger if we had not measured the government's vote share in relation to total *eligible* votes but to total votes *validly cast*.
Also the size and significance of the coefficients of our control variables decreases.

[165] Similarly to table 2, replacing the FL dummy with a "below mean" dummy, a "below median" dummy, or a continuous "gross income" variable equally results in smaller coefficients and lower significance also when adding legislature dummies interacted with resp. the "below mean" dummy, the "below median" dummy, or the continuous "gross income" variable.

[166] A criticism of our approach in table 3 could be that once we interact one variable with the FL dummy, we should interact *all* variables with the FL dummy, as e.g. Brooks e.a. (2011 p. 9) do with their variable of interest. Although this is a demanding specification, interacting all variables with the FL dummy (regression output not shown) does not considerably affect our coefficients of interest throughout this paper.
However, a split sample approach produces rather *different* results, at the cost of more or less halving our number of observations. When running the regression behind column (4) for Flemish districts only, the coefficient on transfers is positive but small and insignificant, while when running the regression behind column (4) for Francophone districts only, the coefficient on transfers is positive, larger and very significant (p value of 0.000). The split sample approach however produces *similar* IV regression results (see below), but evidently the reduced number of observations poses an even bigger handicap to an IV regression than to a simple FD regression. The split sample approach again produces similar results when regressing our dependent variable *directly* on our IVs (again see below).

Table 3: FD regression of the government's vote share on transfers, with the interaction of the legislature dummies with the FL dummy added as extra controls

	(1) D.govt vote	(2) D.govt vote	(3) D.govt vote	(4) D.govt vote
D.over 60			-1.129*	-1.089*
			(0.643)	(0.643)
D.unemployed			0.561	0.629
			(0.607)	(0.600)
D.under 18			0.00680	-0.0270
			(0.806)	(0.796)
D.gross income			0.219***	0.214***
			(0.0793)	(0.0789)
D.ministers			-0.338	-0.357
			(0.315)	(0.304)
D.transfers	**0.204****	**0.249****	**0.488*****	**0.505*****
	(0.0928)	**(0.110)**	**(0.0993)**	**(0.114)**
D.transfers*FL		**-0.299**		**-0.174**
		(0.325)		**(0.345)**
Legislature*FL dummies included as extra controls in all 4 columns				
N	215	215	215	215

Robust standard errors clustered at the district level in parentheses; legislature dummies *as well as legislature*FL dummies* included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2. Instrumental variables (IV) regression results

Of course transfers may be endogenous to popularity of the government, as well as to other time variant factors that we would have failed to include as control variables[167]. In particular we could think of reverse causality between government popularity and transfers. E.g. the Federal governments over our sample period included both the Francophone Belgian and Flemish socialist parties[168], which are assumed to be in favour of an expansion of the "welfare state". Hence transfers may have increased (decreased) whenever governing parties increased (decreased) their vote share, as the latter may have meant an increase (decrease) in the socialist vote[169], and hence an increase (decrease) in

[167] It should be noted that the formula-based nature of our transfers variable as such already mitigates its endogeneity, as formula-based transfers may be assumed to be less easily targetable to particular geographic areas. Virtually the entire distributive politics literature is about discretionary transfers and investment spending, rather than about formula-based transfers.
Another mitigation of endogeneity of our independent variable of interest with respect to government popularity is that it largely consists of social security contributions and expenditures. The latter may be labelled entitlements, as they are hard to scale back after approval, and even could be considered "to start leading a life of their own" (as argued already above). Moreover, in Belgium, as in several other countries, political parties have to an important extent ceded control over the social security system to labour unions and employers' organisations.
[168] Exception made for the 2007-10 coalition, which excluded the Flemish socialists.
[169] Over our entire sample period, an increase (decrease) in the *joint* vote share of the governing parties implied an increase (decrease) in the vote share of the governing parties *taken separately*. The only exceptions are the following: decrease in the vote share of both Green parties in 2003; increase in the vote share of the

socialist intra-coalition power. However, if the latter causality chain held true, we would expect the coefficient of our endogenous variable of interest to be *positive*, while in table 2 its sign is negative.

But we could also think of examples of reverse causality implying a *negative* relationship running from government popularity to transfers. E.g. with the vote losses of *previous* Federal government parties in mind, current Federal government parties may increase transfers as an attempt to avoid or limit their vote loss during the next federal election[170]. Furthermore, current Federal government parties may –correctly- *expect* vote losses in certain districts during the *next* elections, and therefore increase transfers to these districts. Also, Flemish parties that became part of the Federal government over our sample period may have been *less pro-transfers in the first place* compared to Francophone Belgian parties that became part of the Federal government over our sample period[171]. For this reason, any increase in the vote share of Flemish government parties may have been associated with a decrease in transfers to Flemish districts[172].

If the associations of interest in tables 2 and 3 were plagued by reverse causality issues –as seems well possible-, the statistically significant associations between government popularity and fiscal transfers uncovered in tables 2 and 3 would remain interesting and relevant from a political economics point of view. However, in this investigation we are only interested in the one-way –and geographically disaggregated- effect of fiscal transfers on government popularity, and not in the reverse effect. Therefore we need to solve potential endogeneity problems.

To this effect, we resort to an IV approach. We propose 2 different IVs for transfers, thereby hoping to reduce the chance that none of our IVs is fully exogenous to our dependent variable, or to other time variant factors correlated with transfers that we would have failed to include as control variables. Both IVs proposed are *subsets* of overall transfers considered[173]:

1) Joint early retirement due to downsizing 2007-11: The upper 2 curves in graph 3 show retirement benefits received per capita in Flanders resp. in Francophone Belgium due to joint early retirement following the downsizing of a firm. Belgian law foresees the possibility of granting retirement benefits at a younger age than the statutory retirement age in a number of circumstances, among which the downsizing (or closure) of a firm. Such downsizing (or

---

Francophone Belgian liberals in 2007; increase in the vote share of the Francophone Belgian socialists in 2010; decrease in the vote share of the Francophone Belgian socialists in 2014.

[170] Although the current Federal government parties may precisely be those parties that *benefited* from the losses of the previous government parties, due to which the current Federal government may *not* be worried so much about its popularity in districts in which the previous Federal government suffered a vote loss.

[171] In Belgium, it is rather widely accepted that Francophone Belgian parties are more pro-government and more pro-transfers (both in terms of high expenditures and high taxes) than Flemish parties, even when comparing parties of the same "family". E.g. the Francophone Belgian Christian-democrats are believed to be more pro-government and more pro-transfers than the Flemish Christian-democrats.

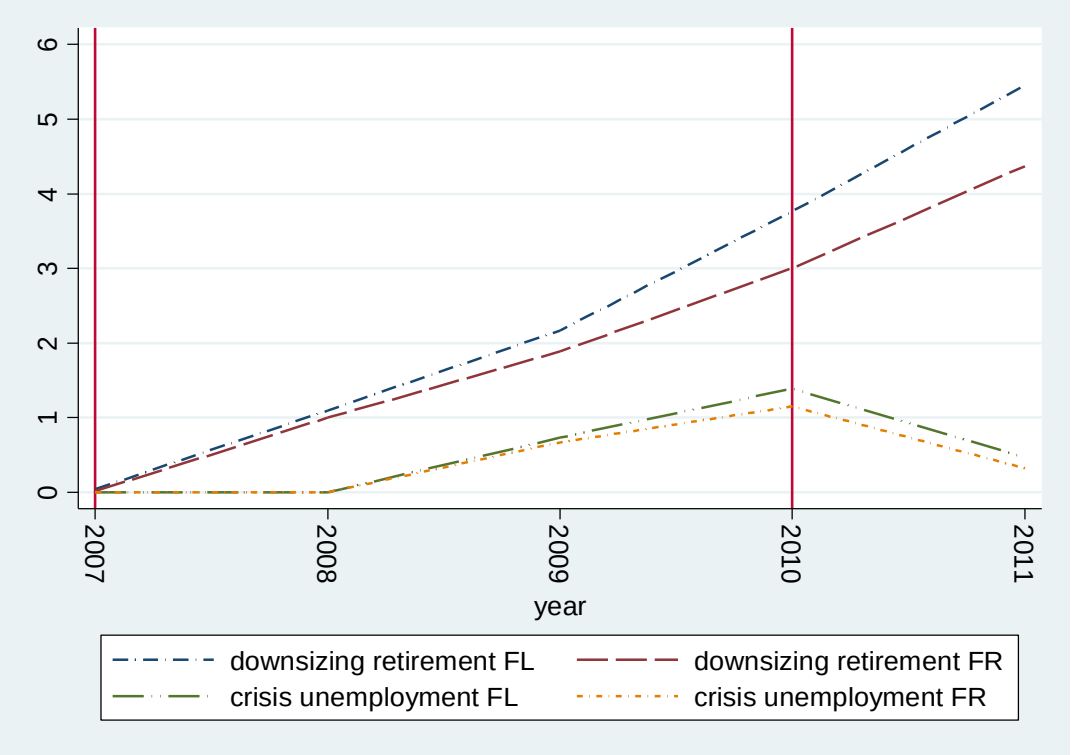
[172] However, the formula-based nature of our transfers variable renders this reasoning unlikely, as this nature seems to make it rather hard to increase or cut transfers to one of the 2 main language communities only, though there exist some examples to the contrary, as given above. Another problem with this reasoning is that the vote share of Flemish government parties *decreased* at the end of most of the legislatures comprised by our sample period.

[173] Moreover they are subsets of only one of the 3 components of our transfers variable, i.e. of the social expenditures component.

For examples of other IV regressions using subsets of the original endogenous variable as an IV, see Dahlberg e.a. (2008) and Allers and Vermeulen (2013). Dahlberg e.a. (2008) use grants received by a Swedish municipality due to outmigration out of that municipality as an IV for total grants received by that Swedish municipality. Allers and Vermeulen (2013) instrument general grants received by Dutch municipalities with the change in grants that is induced by a reform of the general grant allocation.

closure) can be argued to be exogenous to popularity of the then government, as well as to other time variant factors correlated with transfers that we would have failed to include as control variables[174]. The Ministry of Employment has started collecting data per district with respect to joint early retirement due to downsizing only from 2007 onwards[175]. Graph 3 shows that –at least since 2007- Flanders has benefited relatively more from this early retirement possibility than Francophone Belgium. This may have been because the private sector accounts for a larger share of the economy in Flanders than in Francophone Belgium, or because the share of the employed is larger in Flanders than in Francophone Belgium in the first place. A comparison of graph 1 and graph 3 also makes clear that benefits from joint early retirement due to downsizing make up only a small fraction of total transfers (as graph 3 is expressed in euros per capita while graph 1 is expressed in 100 euros per capita).

Graph 3: Joint early retirement benefits due to downsizing 2007-11 and temporary unemployment benefits due to the economic crisis 2009-11 (in real euros of 2011 per capita)



Note: bars indicate federal election years

Source: Federal Ministry of Employment

[174] A criticism of the use of the early retirement benefits due to downsizing as an IV could be that an increase in vote share –and the consequent coming to power or strengthening of their position within the government- of particular –e.g. business unfriendly- parties could encourage firms to downsize. Additionally, certain –e.g. workers friendly- parties may be more lenient than other parties in granting early retirement –both in terms of eligibility and in terms of the financial amounts concerned- on the occasion of a firm's downsizing. However, judged by their party political composition, all Federal government coalitions over our sample period were more or less as worker friendly. Another criticism is that in Belgium, the early retirement age on the occasion of a firm's downsizing is negotiated with the Federal government on a case by case basis. Therefore this age may vary from firm to firm. There are anecdotes that some government ministers have become personally involved in the settlement of closures occurring in their electoral district.

[175] This is why the related curves in graph 3 show a continuous *increase*: the joint early retirement benefits that were granted before 2007 are not shown but some of them may have expired since 2007.

2) Temporary unemployment due to the 2009 economic crisis 2009-11: The lower 2 curves in graph 3 show unemployment benefits received per capita in Flanders and in Francophone Belgium due to temporary unemployment caused by the 2009 economic crisis. Because of the 2009 economic crisis, Belgian law –similar to measures taken in other EU countries- offered employers the possibility to postpone laying off redundant workers and instead to grant them government funded temporary unemployment benefits. Such temporary unemployment can be argued to be exogenous to popularity of the then government, as well as to other time variant factors correlated with transfers that we would have failed to include as control variables, as arguably the 2009 economic crisis was a worldwide phenomenon[176]. Graph 3 shows that Flanders benefited relatively more also from this early retirement possibility than Francophone Belgium. This may once again have been because the private sector accounts for a larger share of the economy in Flanders than in Francophone Belgium or because the share of the employed is larger in Flanders than in Francophone Belgium. Graph 3 also makes clear that also benefits from temporary unemployment make up only a small fraction of total transfers.

Table 4 shows our IV regression results. Following Dahlberg e.a. (2008 p. 2324), we make sure to keep controlling for the share of unemployed also in our IV regression, as our instruments may also *directly* affect government popularity –i.e. *not* via our transfers variable-, because an increase in early retirement and an increase in temporary unemployment imply an increase in the overall number of unemployed, which we assume to *directly* affect government popularity[177]. Columns (1) to (4) show the results when instrumenting transfers with benefits due to early retirement following downsizing, in columns (1) and (2) without the interaction of our transfers variable with the FL dummy, and in columns (3) and (4) with the interaction of our transfers variable with the FL dummy[178]. The associations between transfers in general and transfers to Flemish districts in particular on the one hand and the government's vote share on the other hand are broadly similar in sign and significance to the associations in columns (3) and (4) of table 2. However, the size of the associations is considerably larger than their size in table 2, as their size is generally considerably larger in all other IV regressions below. This larger size may be due to the fact that IV estimation removes the downward bias of OLS estimates due to measurement error.

Columns (1) and (2) confirm the absence of any association between transfers in general and government popularity. The difference between columns (1) and (2) is that column (2) adds the lagged dependent variable (LDV) as a regressor. This inclusion is inspired by the correlation between

[176] A criticism of the use of temporary unemployment benefits due to the economic crisis as an IV could be that an increase in vote share –and the consequent coming to power- of particular –e.g. worker friendly- parties could increase the probability of a temporary unemployment benefits scheme being established, as well as of the related benefit amounts granted being more generous. Indeed, the then Federal government coalition expanded the decades' old Belgian temporary unemployment benefits scheme in 2009 on the occasion of the then economic crisis, to enable not only blue-collar workers but also white-collar workers to benefit from it. However, arguably the then government coalition was more or less as worker friendly as the previous coalition, judged by its party political composition.

[177] An increase in early retirement and an increase in temporary unemployment imply an increase in the overall number of unemployed, either because the early retired and the temporary unemployed are simply a subset of the overall unemployed, or because it is well possible that whenever in a particular district the number of early retired or the number of temporary unemployed increase, also "ordinary" unemployment increases, e.g. because not all workers laid off may be eligible for early retirement or temporary unemployment. (This point was made clear to me by Filip Bossier and Hilde Geeraers of the Ministry of Employment.)

[178] Regression results in columns (1) to (4) are similar when using the *number* of beneficiaries rather than the *amount* of benefits due to early retirement following downsizing as an IV.

successive differences in government parties' joint vote share suggested by graph 2. We instrument the LDV with the 2nd and 3rd lag of the dependent variable expressed in levels[179]. The larger size of our coefficients of interest compared to table 2 is even less surprising in column (2), because when including an LDV, the coefficient on transfers only measures the short-term effect of a change in transfers on government popularity, which may reasonably be assumed to be higher than the "average" effect. In column (3) of table 4 the coefficient on our interacted transfers variable turns insignificant compared to column (4) of table 2, and additionally the F-test of joint significance points at a relevance of 1 of our IVs that is problematic. However, when we add the LDV as a regressor, as done in column (4), our coefficient of interest almost turns significant again (p value of 0.16). A disadvantage of including the LDV as a regressor is that it makes us lose 2 more legislatures of observations. This is because we need to instrument the LDV -as said by making use of the 2nd and 3rd lag of the dependent variable expressed in levels-, which reduces our number of legislatures to a mere 3, and our number of observations to a mere 129.

Columns (5) to (8) show the IV regression results when using benefits due to temporary unemployment following the 2009 economic crisis as an IV, in columns (5) and (6) without the interaction of our transfers variable with the FL dummy, and in columns (7) and (8) with the interaction of our transfers variable with the FL dummy[180]. Once again, the associations between transfers on the one hand and the government's vote share on the other hand are broadly similar in sign and significance to the associations in columns (3) and (4) of table 2. Again, columns (5) and (6) confirm the absence of any association between transfers in general and government popularity.

The difference between the regressions using early retirement due to downsizing as an IV and the regressions using benefits due to temporary unemployment following the 2009 economic crisis as an IV is that in the latter regressions the coefficient on the interacted transfers variable is much more significant (columns (7) and (8)), similar to column (4) in table 2. Moreover, in column (8) we can additionally conduct the Hansen J test, as we now have available one more excluded instrument than instrumented variables, thanks to instrumenting the LDV with the 2nd and 3rd lag of the dependent variable expressed in levels. The Hansen J test shows us that we cannot exclude validity of the IV. Moreover, the F-test of joint significance shows that our IV is relevant.

Therefore we can now claim the association between transfers to a Flemish district and government popularity to consist of a *causal* effect. Thus an increase in transfers to Flemish voters makes the latter vote *less* for government parties rather than more. This result could be interpreted as supportive of our hypothesis that Flemish parties have less of an incentive to run a federal deficit –conducive to an increase in transfers- than Francophone Belgian parties. Flemish voters may be aware that an increase in transfers to them may come at a high cost, keeping in mind 1) the track record of fiscal indiscipline of the Belgian Federal government, as well as 2) the formula-based –i.e. Belgium-wide- and government-to-citizen nature of the transfers considered.

[179] With the risk of comparing apples and oranges: the 2nd as well as the 3rd lag of government popularity may concern different governing parties as well as a different *number* of governing parties compared to the contemporaneous government popularity variable.
An advantage of increasing our number of IVs is that this is expected to reduce our standard errors.

[180] Regression results in columns (5) to (8) are similar when using the *number* of beneficiaries rather than the *amount* of benefits due to temporary unemployment following the 2009 economic crisis as an IV.

Table 4: IV regression of the government's vote share difference on difference in transfers

	IV: D.downsizing retirement benefits				IV: D.crisis unemployment benefits			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote
D.over 60	-9.522	-3.856	6.198	1.466	-4.248**	-7.268***	-4.343*	-1.368
	(13.01)	(4.223)	(54.04)	(2.271)	(1.784)	(2.225)	(2.536)	(2.398)
D.unemployed	-0.852	-1.880	24.72	7.215	0.707	-4.258	6.220***	10.69**
	(3.872)	(3.809)	(37.42)	(5.772)	(0.910)	(3.867)	(1.739)	(4.729)
D.under 18	-7.886	-3.940	19.02	0.629	-4.416**	-6.509**	-0.0925	-0.861
	(8.738)	(4.108)	(50.30)	(2.804)	(1.778)	(3.144)	(2.854)	(3.264)
D.gross income	1.549	-1.355	-6.155	-1.239	0.0227	-0.738	-0.785	0.368
	(3.814)	(1.003)	(18.61)	(0.960)	(0.524)	(0.512)	(0.510)	(0.615)
D.ministers	-0.990	0.157	-2.926	0.706	-0.240	0.261	-1.358	1.136
	(2.124)	(1.528)	(6.630)	(1.733)	(0.675)	(1.663)	(1.131)	(1.891)
D.transfers	**3.663**	**-2.128**	**-6.365**	**-2.019**	**-0.143**	**-0.767**	**0.274**	**1.295**
	(9.448)	**(1.895)**	**(38.66)**	**(2.018)**	**(1.326)**	**(0.978)**	**(1.143)**	**(1.035)**
D.transfers*FL			**-24.51**	**-4.302**			**-6.490*** **	**-7.881*** **
			(28.51)	**(3.068)**			**(0.831)**	**(1.309)**
L.D.govt vote		-0.0836		0.348***		-0.432***		0.0348
		(0.287)		(0.123)		(0.160)		(0.163)
N	215	129	215	129	215	129	215	129
Hansen J (p value)	-	0.18	-	0.04	-	0.63	-	0.22
F-test of joint significance of IVs in 1st stage (p value)	0.41	0.08	0.37	0.15	0.00	0.00	0.00	0.00
Number of IVs	1	3	2	4	1	3	2	4

Robust standard errors clustered at the district level in parentheses; legislature dummies included; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Again, with respect to our IV regressions, one could argue that a lower vote share obtained by the governing parties is caused by a district's transfers being below the mean (or below the median), rather than by a district being situated in Flanders. I.e. in those few Francophone Belgian districts that are characterised by transfers that are below the mean (or below the median), an increase in transfers could well cause the government's vote share to decrease as much as in Flanders. Likewise, in those few Flemish districts that are characterised by transfers that are above the mean (or above the median), an increase in transfers could well cause the government's vote share to remain as stable as in Francophone Belgium. However, when we replace the FL dummy with a dummy "below mean" (or with a dummy "below median") taking the value of 1 in all districts –Flemish or Francophone Belgian- that are characterised by transfers that are below the mean (or below the median), the coefficients of interest in table 4 become smaller and less significant, often even insignificant, similarly to table 2[181]. This suggests that –while being characterised by transfers that are below the mean or the median may cause a district to worry about an increase in transfers- even Flemish districts with transfers *above* the mean (or above the "median") are –rightly or wrongly- more worried about transfers than Francophone Belgian districts. Similarly, this suggests that even Francophone Belgian districts with transfers *below* the mean (or below the median) are –rightly or wrongly- less worried about transfers than Flemish districts[182].

Regression results in table 4 broadly remain unchanged when including the 2004 and 2009 regional election results into our dependent variable. Taking into account the results of the 2004 and 2009 regional elections may be warranted because considering federal legislatures as time periods rather than individual years goes with the disadvantage of losing a lot of observations. A way to increase our number of time periods available again is to take into account the Federal government parties' vote share during *non-concurrent regional elections*. Indeed, regional elections in Belgium may well have been a test of the popularity of the *Federal government* coalition as well[183]. Regression results in table 4 also broadly remain unchanged when using the –differed- "non-system vote" as the dependent variable. We define the "non-system" vote as the difference between 100% and the vote share obtained by parties that participated in at least 1 Federal government over our sample period. The "non-system vote" may be a better measure of voters' dislike of transfers –and of debt increases that may follow from it- than votes for the governing coalition. This is because votes for opposition parties that are "system parties", i.e. that *did* govern during at least one legislature over our sample period, may not so much express dislike of transfers *as such*, rather than dislike of the *current distribution* of transfers[184].

All in all, our IV regression results of table 4 confirm our finding of a negative association between transfers to Flemish districts and popularity of Flemish government parties in column (4) of table 2. The results using temporary unemployment benefits additionally show a negative causal effect of transfers to Flanders on Flemish governing parties' vote share.

---

[181] The association also becomes smaller and less significant when we replace the FL dummy with the continuous "gross income" variable.

[182] Replacing the FL dummy with a "below mean" (or "below median") dummy or with the continuous "gross income" variable equally results in smaller coefficients and lower significance when adding legislature dummies interacted with the FL dummy (see below).

[183] Adding the election results of the 2004 and 2009 regional elections equally results in smaller coefficients and lower significance compared to the FD regression results when adding legislature dummies interacted with the FL dummy.

[184] Using the "non-system vote" as the dependent variable equally results in smaller coefficients and lower significance compared to the FD regression results when adding legislature dummies interacted with the FL dummy.

However, adding the interaction of the legislature dummies with the FL dummy as extra control variables -as done in table 5- again changes our regression outputs considerably. Columns (1), (2), (5) and (6) of table 5 show that the significantly positive association between transfers in general and government popularity as found in column (3) of table 3 disappears. Additionally, as a result of the specifications behind columns (3), (4), (7) and (8), hardly any of our 2 coefficients of interest is significant anymore. Columns (4) and (8) even show an unexpected positive sign for the interacted variable (but it should be added that these columns show the results of a regression merely using 129 observations due to the inclusion of an LDV). Also the F-test now points at one or more weak IVs in most specifications[185].

[185] Similarly, when replacing net transfers with its 3 constitutive *components* –social transfers, social security contributions, and income tax payments- as the dependent variable both in a simple FD and in an IV regression, we find a significantly negative association between transfers to Flanders and government popularity, while this effect disappears once we include legislature dummies interacted with the FL dummy. Indeed, a criticism of using a composite net transfers variable as our independent variable could be that its *social security contributions* component may be less visible to the citizen, as it is directly paid by his employer to the government –i.e. a citizen's salary received is net of social security contributions. Moreover, the pay slip of an employee does not even mention the so-called "employer's contribution" to social security that an employer makes per employee, while this "employer's contribution" is well included into the social security contribution component of our net transfers variable.

Table 5: IV regression of the government's vote share difference on difference in transfers, with the interaction of the legislature dummies with the FL dummy added as extra controls

	IV: D.downsizing retirement benefits				IV:D.crisis unemployment benefits			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote	D.govt vote
D.over 60	-6.774	-1.413	-1.951	-2.124	-0.697	-0.756	0.326	-1.159
	(9.351)	(1.470)	(1.707)	(2.392)	(0.735)	(1.043)	(2.055)	(1.290)
D.unemployed	-2.230	3.111	0.916	1.565	0.774	0.143	2.328	-0.231
	(4.897)	(2.135)	(1.632)	(3.167)	(0.618)	(1.854)	(2.288)	(1.984)
D.under 18	-3.146	0.0798	-1.334	0.149	0.248	0.626	-0.394	0.529
	(5.095)	(1.876)	(1.058)	(2.852)	(0.853)	(1.611)	(1.206)	(2.149)
D.gross income	2.763	1.090	0.791	1.119	0.0241	-0.00379	-0.160	0.144
	(4.276)	(0.730)	(0.496)	(0.928)	(0.196)	(0.188)	(0.467)	(0.222)
D.ministers	-1.239	-1.416*	-0.862	-2.585	-0.269	-0.000231	-0.647	-0.863
	(1.793)	(0.821)	(0.566)	(1.714)	(0.290)	(0.940)	(0.587)	(1.399)
D.transfers	**6.651**	**2.067**	**2.335****	**1.756**	**0.0166**	**0.202**	**0.206**	**0.255**
	(10.25)	**(1.429)**	**(0.994)**	**(1.686)**	**(0.417)**	**(0.292)**	**(0.462)**	**(0.380)**
D.transfers*FL			**-2.741**	**3.645**			**-3.773**	**1.910**
			(3.449)	**(4.035)**			**(4.536)**	**(2.347)**
L.D.govt vote		8.145**		8.909*		0.00148		1.559
		(4.110)		(5.111)		(2.589)		(2.904)
Legislature*FL dummies included as extra controls in all 8 columns								
N	215	129	215	129	215	129	215	129
Hansen J (p value)	-	0.31	-	0.91	-	0.06	-	0.05
F-test of joint significance of IVs in 1st stage (p value)	0.56	0.33	0.18	0.16	0.00	0.59	0.29	0.60
Number of IVs	1	.3	2	4	1	3	2	4

Robust standard errors clustered at the district level in parentheses; legislature dummies *as well as legislature*FL dummies* included; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.3. *Direct* FD regression of the government's vote share on our IVs

Because our 2 IVs are simply *subsets* of our overall variables of interest, we would expect the coefficients on our IVs (as opposed to most other IVs) to be significant with the expected sign and to be ready for interpretation in a simple *direct* FD regression of the dependent variable (in our case the governing coalition's vote share) on our IVs. Of course in such a regression we again should take care to control for the number of unemployed, as we wish to estimate the effect of the *transfers* that follow from early retirement due to downsizing and temporary unemployment due to the 2008-9 economic crisis, and *not* the effect of the *unemployment* as such these 2 events cause[186]. Such a direct regression can be considered as an alternative to our IV regression for which we presented the results in table 4 above[187]. Table 6 presents the results of *directly* regressing the government's popularity on our 2 IVs – transfers ascribed to resp. the downsizing of firms and the impact of the 2009 worldwide economic crisis on Belgium- consecutively.

The direct regressions on transfers now confirm our results obtained in table 4: no effect of transfers in general and a significantly smaller –and even negative- effect of transfers to Flanders separately. Moreover in the specification with an interacted transfers variable also the coefficients of the non-interacted transfers variable now become significant (with the expected sign), implying a positive effect of transfers to Francophone Belgium on government popularity. While the coefficients of interest in table 6 are much larger, the typical effect is of the same order of magnitude as the typical association of the simple FD regression in table 2 above. We calculate the typical effect by means of example for the interacted variable of interest in column (2). The within standard deviation of early retirement benefits per Flemish district over our 6 legislatures is 2.5 euros per capita. Therefore, the typical variation in the difference in government vote share between a Francophone and a Flemish district due to variations in early retirement benefits – (a bit unrealistically) expressed in 100 euros per capita- over time equals about -3.6%points (-142.0%points times 0.025), with a p value of 0.000 (compared to -3.7%points in table 2).

[186] However, the unemployment control hardly seems to affect our coefficients of interest reported in tables 6 and 7, as the latter hardly change when we omit the unemployment control from each of the regression specifications reported in tables 6 and 7.

[187] A problem with respect to our first instrument –early retirement benefits due to downsizing- could be that –as graph 3 implies- its value is zero for most of our observations, as the Ministry of Employment only started collecting data for our first instrument from 2007 onwards. In reality however, early retirement benefits due to downsizing were always strictly positive in each year of our sample period. However, if we would consider early retirement benefits per district pre-2007 as missing observations, this would drastically reduce our sample size. This problem evidently does not apply to our 2nd instrument: the extra unemployment benefits granted because of the 2009 economic crisis are of course effectively zero pre-2009.

Table 6: *Direct* FD regression of the government's vote share on our IVs

	(1) D.govt vote	(2) D.govt vote	(3) D.govt vote	(4) D.govt vote
D.over 60	-4.363***	-3.654***	-4.433***	-3.686***
	(1.168)	(1.078)	(1.085)	(0.971)
D.unemployed	0.610	1.465**	0.647	1.238*
	(0.765)	(0.645)	(0.763)	(0.682)
D.under 18	-4.383**	-3.443*	-4.521**	-3.282**
	(1.929)	(1.766)	(1.758)	(1.533)
D.gross income	0.0785	0.0759	0.0803	0.160*
	(0.110)	(0.0991)	(0.109)	(0.0839)
D.ministers	-0.253	-0.116	-0.276	-0.333
	(0.660)	(0.546)	(0.688)	(0.539)
D.downsizing retirement benefits	**-11.78**	**90.62*** **		
	(26.36)	**(28.06)**		
D.downsizing retirement benefits*FL		**-142.0*** **		
		(20.62)		
D.crisis unemployment benefits			**-17.77**	**520.7*** **
			(171.4)	**(176.2)**
D.crisis unemployment benefits*FL				**-1114.2*** **
				(137.0)
N	215	215	215	215

Robust standard errors clustered at the district level in parentheses; legislature dummies included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Adding the interaction of the legislature dummies with the FL dummy as extra control variables -as done in table 7- weakens the evidence provided by our regression outputs in favour of our hypothesis, but less so than in the IV analysis shown in table 5. Compared to table 6, the specifications in table 7 broadly show the same sign for the coefficients of our 2 variables of interest. But compared to columns (2) and (4) in table 6, the coefficients of the non-interacted variable of interest are no longer significant, and in column (4) also the coefficient of the interacted variable of interest fails to be significant with a p value of 0.15. Additionally, the sizes of the coefficients on the interacted variables in table 7 are small. E.g. the coefficient of -5.33 on the interacted variable in column (2) implies a typical difference in effect of a 100 euros increase in early retirement benefits in Flanders on government popularity amounting to -0.14%points only (with a p value of 0.000).

Table 7: *Direct* FD regression of the government's vote share on our IVs, with the interaction of the legislature dummies with the FL dummy added as extra controls

	(1) D.govt vote	(2) D.govt vote	(3) D.govt vote	(4) D.govt vote
D.over 60	-0.589	-0.685	-0.683	-0.756
	(0.702)	(0.705)	(0.693)	(0.700)
D.unemployed	0.720	0.885	0.782	0.804
	(0.642)	(0.646)	(0.633)	(0.634)
D.under 18	0.440	0.414	0.254	0.193
	(0.931)	(0.929)	(0.880)	(0.890)
D.gross income	0.0143	0.00566	0.0173	0.0240
	(0.0784)	(0.0772)	(0.0765)	(0.0779)
D.ministers	-0.248	-0.183	-0.266	-0.281
	(0.303)	(0.313)	(0.306)	(0.308)
D.downsizing retirement benefits	**-1.397**	**1.971**		
	(1.148)	**(1.514)**		
D.downsizing retirement benefits*FL		**-5.332*** **		
		(1.860)		
D.crisis unemployment benefits			**0.204**	**7.959**
			(5.399)	**(7.231)**
D.crisis unemployment benefits*FL				**-16.77**
				(11.33)
Legislature*FL dummies included as extra controls in all 4 columns				
N	215	215	215	215

Robust standard errors clustered at the district level in parentheses; legislature dummies *as well as legislature*FL dummies* included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As an extra check we repeated the regressions for which the results are shown in tables 6 and 7 after adding our original endogenous transfers variable as well as our original endogenous transfers variable interacted with the FL dummy as extra controls. This is to conduct an extra check if it is *the transfers* due to early retirement because of downsizing as well as *the transfers* due to temporary unemployment because of the economic crisis that have the direct effect on government popularity apparent from tables 6 and 7, rather than *the events causing these transfers*. The regression results (not shown) then still show the same coefficients on our –presumably exogenous- variables of interest to be significant as in tables 6 and 7. These results confirm our fear that it could well be *the events behind our IVs, rather than the transfers captured by our IVs*, that directly impact our dependent variable government popularity. These results suggest that voters –rightly or wrongly- ascribed the downsizing of firms and/or the size of the impact of the 2009 worldwide economic crisis on Belgium to the Federal government in office at those times. It could therefore well be voters' *increased disappointment* with a government possibly held responsible for the investors climate and/or the economic cycle[188] rather than the –related- *increase* in transfers that explains the decreased government popularity[189].

[188] Notwithstanding the fact that we control for the share of unemployed.

6. Conclusion

As political parties in Belgium are split along Flemish-Francophone linguistic lines, and as Flanders enjoys relatively less federal expenditures and pays relatively more federal taxes than Francophone Belgium, the incentives for Flemish parties to run a budget deficit when governing at the federal level seem smaller than for Francophone parties. As the federal debt is serviced out of federal tax revenues, Flanders bears most of the cost of debt servicing, while having benefited relatively less from the additional expenditures that had been made possible by a federal budget deficit.

The geographically unevenly spread benefits of Belgian federal budget deficits –and the geographically diverging incentives for federal budget deficits this disparity seems to create- may well explain to some extent the large overall size of the federal debt since the 1970s. Therefore, interregional fiscal transfers resulting from the federal debt may be important not just because of their distributional *consequences*, but even more because they may provide an *explanation* for the large overall size of deficits and debt that have characterised Belgium since the 1970s[190].

We have tested for these differing federal budget deficit incentives empirically by investigating the effect of an increase in net fiscal transfers to inhabitants of the 22 Flemish districts on the popularity of Flemish parties that govern at the federal level, compared to the effect of an increase in net fiscal transfers to inhabitants of the 21 Francophone Belgian districts on the popularity of Francophone parties that govern at the federal level, over the 1995-2011 period. Our net fiscal transfers variable is formula-based and is defined as social transfers received by citizens minus social security contributions and income taxes paid by citizens. We consider our net fiscal transfers variable as an –imperfect- indicator to citizens of their district's contribution to the federal budget balance.

In contrast to the existing distributive politics literature, we have found that an increase in net fiscal transfers to Flemish districts causes Flemish parties governing at the federal level to *lose* votes during the next federal election, while the effect is insignificant for Francophone Belgian parties as well as for Belgium as a whole. This negative effect of net transfers on votes in Flanders is remarkable, all the more so because our net transfers variable consists more of *transfers paid by citizens to the Federal government* (social security contributions and income taxes) *than of transfers received by citizens* (social transfers). The negative effect may be suggestive of Flemish voters being aware of the facts (1) that increases in formula-based and government-to-citizen net transfers to Flanders may go hand in hand with –possibly even larger- increases in the same net transfers to Francophone Belgium, (2) that increases in these transfers may increase the public debt, and (3) that the public debt is serviced disproportionately with federal taxes collected in Flanders. Our findings seem consistent with our earlier calculations that Flanders bears a far larger share of the tax cost of Belgian budget deficit accumulation than Francophone Belgium.

Such disappointment could well have been shared by voters who did *not* lose their job, as these may have feared that they would be "next in line".

[189] Alternatively, decreased government popularity could simply have been caused by the possibly *changed perception of interests* of the newly retired and/or the newly unemployed. This latter explanation seems somewhat improbable however, as the interests of both workers and beneficiaries of retirement and unemployment benefits seem to be best defended by socialist and Christian-democratic parties, which were part of the Federal government coalition over most of our sample period.

[190] Indeed, while cooperation of *all* coalition partners may be needed to *avoid* a deficit, non-cooperation by *one* of them may suffice to *cause* a deficit. See Roubini and Sachs (1989).

Could the difference in the effect of transfers on government popularity between Flanders and Francophone Belgium be due to a simple *difference in their preferences* for government spending[191]? This does not seem likely, as those preferences probably did not substantially change over our sample period, and as the bulk of our transfers variable consists of government *revenues*, not spending. Could the difference in the effect of transfers on government popularity between Flanders and Francophone Belgium be due to the considerable and widening difference in their gross incomes per capita[192], and/or in their other demographic-economic characteristics? This does not seem likely either, as we control for gross income per capita, as well as for other demographic-economic characteristics.

The negative effect is however smaller and less significant when weighting our net transfers variable for the size of the federal budget deficit. A possible explanation is that for the individual citizen net fiscal transfers received at the district level may be a more visible indicator of the sustainability of federal public finances than the aggregate federal budget balance. Another explanation would be that Flemish voters dislike extra formula-based transfers to Flanders *even if the federal budget would be balanced*. Indeed, Flemish voters may fear that the concomitant extra formula-based transfers to Francophone Belgium would be even larger or would be funded with less federal expenditures to or more taxes raised from Flanders that are *not* included in our independent formula-based net transfers variable. In this case, our regression analysis would be about measuring the effect of net fiscal transfers *as such* on government popularity, rather than on the effect of federal budget deficits on government popularity.

The Flemish negative effect of transfers received on government popularity even turns insignificant in a number of specifications when adding *Flanders-specific* legislature fixed effects as control variables, while this addition preserves the insignificantly positive Francophone Belgian as well as Belgium-wide effect in most regression specifications. Adding *Flanders-specific* legislature fixed effects as control variables may be justified because federal elections in Belgium are held in parallel in Flanders and in Francophone Belgium, with the 2 communities hardly having any geography, media, language or political parties in common.

Therefore, while -fiscally speaking- Francophone Belgian parties would have an incentive to run a deficit while the reverse holds for Flemish parties, we only find weak evidence that these incentives would be reflected into the behaviour of Francophone Belgian or of Flemish voters. We therefore cannot conclusively state that Francophone Belgian voters would particularly reward extra net transfers received with more votes, nor that Flemish voters would reward extra net transfers received with significantly less votes. Perhaps our inconclusiveness means that Francophone Belgian voters

[191] In Belgium it is widely accepted that Francophone Belgian voters are more pro public expenditures than Flemish voters. E.g. over our sample period a much larger share of Francophone Belgian voters voted for leftwing parties compared to Flanders. Of the Francophone Belgian parties that obtained parliamentary seats over our sample period, PS, Ecolo, and cdH are considered leftwing. (cdH is only being considered leftwing – instead of centre- after 1999, the year in which cdH was expelled from the Federal government for the 1st time since WWII.) In the parliament of the French Community, these 3 parties combined have held between 66 and 73% of the seats since 1999. This is a far higher share than the combined share of the Flemish parties that are considered leftwing, i.e. sp.a and Groen. The Francophone Belgian leftwing vote share remains higher if we also count votes for CD&V, the Flemish centre party, as leftwing votes.

[192] The difference between the average annual gross income per capita in Flanders and in Francophone Belgium increased from 2123 to 2775 real euros between 1995 and 2011.
Dixit and Londregan (1998) hypothesize that the votes of lower-income groups can more easily be "bought" with transfers.

overestimate the future cost of extra transfers they will bear[193], and that Flemish voters underestimate the future cost of extra transfers they will bear. Alternatively, our inconclusiveness may mean that Francophone Belgian parties overestimate the rewards -in terms of voters' gratitude- of steering extra transfers to Francophone Belgium, and that Flemish parties correctly estimate the –lack of- relative punishment of steering extra transfers to Flanders. This lack of punishment may explain the continuous federal budget deficits and the high federal public debt in Belgium.

From the perspective of the existing literature on the impact of transfers on government popularity, the weak evidence found regarding a positive Francophone Belgian and Belgium-wide effect seems more puzzling than the weak evidence found regarding a less positive Flemish effect.

References

Alesina, A. and R. Perotti, "The political economy of budget deficits". NBER working paper, 1994.

Alesina, A. and G. Tabellini, "Positive and normative theories of public debt and inflation in historical perspective", European Economic Review 39 (1992): 337-344.

Allers, M. and E. Merkus, "Soft budget constraint but no moral hazard? The Dutch local government bailout puzzle". University of Groningen working paper, 2013.

Allers, M. and W. Vermeulen, "Fiscal Equalization and Capitalization: Evidence from a Policy Reform". CPB Discussion Paper, 2013.

Ansolabehere, S. and J. Snyder (2002), "The incumbency advantage in US elections. An analysis of state and federal offices 1942-2000", Election law journal 1 (2002): 315-338.

Barro, R., "Are Government Bonds Net Wealth?", Journal of Political Economy 82 (1974): 1095-1117.

Bracco, E., F. Porcelli, and M. Redoano, "Incumbent effects and partisan alignment in local elections: a regression discontinuity analysis using Italian data". Working paper, 2012.

Brollo, F. and T. Nannicini, "Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil", American Political Science Review 106 (2012): 742-761.

Brooks, L., J. Phillips, and M. Sinitsyn, "The cabals of a few or the confusion of a multitude: the institutional trade-off between representation and governance", American economic journal: Economic policy 3 (2011): 1-24.

Curto-Grau, M., A. Solé-Ollé and P. Sorribas-Navarro, "Partisan targeting of intergovernmental transfers and state interference in local elections. Evidence from Spain". Working paper, 2012.

Dahlberg, M. and E. Johansson, "On the vote-purchasing behavior of incumbent governments", American Political Science Review 96 (2002): 27-40.

[193] However, because gross income of Francophone Belgian voters is on average lower than gross income of Flemish voters, Francophone Belgian voters may attach a higher weight to one euro paid in terms of future taxes to service the debt stemming from a current increase in net transfers.

Dahlberg, M., E. Mörk, J. Rattsø, and H. Ågren, "Using a discontinuous grant rule to identify the effect of grants on local taxes and spending", Journal of Public Economics 92 (2008) 2320-2335.

de Haan, J. and E. Sturm, "Political and economic determinants of OECD budget deficits and government expenditures. A reinvestigation", European Journal of Political Economy 13 (1997): 739-750.

De Standaard, "Meer bedrijfswagens in Vlaanderen", 3 May 2014.

Dixit, A. and J. Londregan, "Ideology, tactics and efficiency in income redistribution", Quarterly Journal of Economics 113.2 (1998): 497-529.

Dubay, C., "Federal tax burdens and expenditures by state: which states gain most from Federal fiscal operations?" Tax Foundation, Special Report no. 139, 2006.

HRF, "Advies betreffende de schommelingen van de verdeelsleutel van het aan de gewesten toegewezen gedeelte van de personenbelasting", 2010.

Huber, G., M. Kocher and M. Sutter, "Government strength, power dispersion in governments and budget deficits in OECD-countries. A voting power approach", Public Choice 116 (2003): 333-350.

Jennes, G., "Interregional fiscal transfers resulting from the interest bill on the Belgian public debt". KULeuven, Vives briefing, 2014a.

Jennes, G., "Publieke transfers tussen de Belgische gewesten 2007-2011". KULeuven, Vives briefing, 2014b.

Jennes, G. and D. Persyn, "The effect of political representation on the geographic income distribution in Belgium: evidence using Belgian data", European Journal of Political Economy 37 (2015): 178-194.

Knight, B., « Legislative representation, bargaining power and the distribution of federal funds : Evidence from the US Congress », Economic Journal 118 (2008) : 1785-1803.

Lago-Peñas, S., A. Sacchi, and P. Simon-Cosano, "Who honor the rules of Federalism? Party system nationalisation and fiscal performance". Universida de Vigo, GEN working paper, 2014.

Lee, D., "The Electoral Advantage to Incumbency and Voters' Valuation of Politicians' Experience. A Regression Discontinuity Analysis of Elections to the US". NBER working paper, 2001.

Levitt, S., "Using electoral cycles in police hiring to estimate the effect of police on crime", American Economic Review 87 (1997): 270-290.

Lyytikäinen, T. and Tukiainen, J., "Voters are rational". VATT Working Papers 50, 2013.

Migueis, M., "The effect of political alignment on transfers to Portuguese municipalities", Economics & Politics 25 (2013): 110-133.

Persson, T. and G. Tabellini, "Political economics". MIT Press, 2000.

Pettersson-Lidbom, P., "Testing political agency models". University of Stockholm Working Paper, 2006.

Roubini, N. and J. Sachs, “Political and economic determinants of budget deficits in the industrial democracies”, European Economic Review 33 (1989): 903-933.

Spolaore, E. (2008), "Federalism, regional redistribution, and country stability". Tufts University Discussion Paper Series, 2008.

Stratmann, T., “The effects of earmarks on the likelihood of reelection”, European Journal of Political Economy 32 (2013): 341-355.

Veiga, L. and F. Veiga, “Intergovernmental fiscal transfers as pork barrel”, Public choice 155 (2013): 335-353.

Volkerink, B. and J. de Haan, “Fragmented government effects on fiscal policy. New evidence”, Public Choice 109 (2001): 221-242.

Annex 1: "Arrondissements" (districts) of Belgium

Flemish districts are districts to the north of the Belgian language border, i.e. to the north of the quasi-horizontal line running from west to east through the middle of Belgium. The exception is the Brussels district, which we consider to be a Francophone Belgian district.

Francophone Belgian districts are districts to the south of the Belgian language border, plus Brussels.

District names are indicated in bold. Names of provinces are indicated in non-bold. Names of districts that are also the names of provincial capitals are in capital letters.

Chapter 4: Partisan allocation of grants to Brussels municipalities: does it pay to be small and new in power?[194]

Abstract

We investigate the politicisation of intergovernmental grants *per individual party* in a coalition at the municipal level of the Brussels Capital Region of Belgium. We find that municipal coalitions comprising Ecolo, a small party that is moreover relatively new in power, receive significantly more discretionary grants when that party is also in power at one of the subsidising levels. However, we find that these extra grants are not sufficient to compensate for the decrease in grants that municipalities experience when they are governed by this same party Ecolo, if Ecolo is *not* also in power at one or more subsidising levels. Our results are obtained by instrumenting being in power at the municipal level with cases of being in power as part of a coalition from which a dominant party has been excluded, and with cases of 1st ever coalition participation.

Keywords: distributive politics; fiscal federalism; discretionary grants; partisan alignment

[194] I thank Anne-Leen Erauw of Belfius Bank, Lieven Derouck, Danny Geerts, Serge Govaert, the Federal Ministry in charge of the Fund for Large Cities (Stijn Van Wolputte), and the Brussels Region's Ministry in charge of the "contrats de quartier" (Fabienne Lontie) for data provision.
I thank Maarten Allers, Stijn Baert, Thushyanthan Baskaran, Erik Buyst, Peter Claeys, Matz Dahlberg, Stefaan Decramer, Stijn De Ruytter, Andrea Guariso, Joep Konings, Mariana Lopes da Fonseca, Simon Miegielsen, Theo Peeters, Jo Reynaerts, Klaas Staal, David Stadelmann, Wouter Torfs, Robrecht Vandendriessche, Marieke Vandeweyer, Stijn Vanormelingen, Tom Van Puyenbroeck, Wouter Vermeulen and participants to the 2014 IIPF conference in Lugano for advice on the empirical strategy.

1. Introduction

Belgium is a federal country with 3 important layers of government, the Federal government, the Regional and Community governments, and the Municipal governments[195]. Moreover, the Belgian system of fiscal federalism is heavily grants-based, when compared internationally. (see e.g. Jennes 2014)

Grants financing of lower level governments has advantages over its alternative, i.e. tax autonomy for the governments receiving the grants. The most important ones seem that grants from government to government avoid the negative tax externalities that tax autonomy may generate, and that they enable the same level of public goods provision in all jurisdictions. However, intergovernmental grants also have major drawbacks, such as the flypaper effect (i.e. their tendency to increase public spending), the soft budget effect (i.e. their tendency to increase government debts), and politicisation. The recent literature –see e.g. Bordignon e.a. (2013) for Italy and Brollo e.a. (2013) for Brazil- has added 2 more possible disadvantages of intergovernmental grants compared to tax autonomy, i.e. an increase in corruption and a decrease in the quality of politicians –i.e. in their professional other than political skills.

This paper is the first one to our knowledge to investigate the politicisation of intergovernmental grants in Belgium. So far evidence was only anecdotal (for the Brussels municipality level see e.g. Le Vif/L'Express 2011). The paper's attempt is to be part of the 2nd generation papers in fiscal federalism investigating politicisation of grants, i.e. to find a way around the problem of endogeneity of being in power. Grants may not so much be determined by politics, rather it may be that grants determine political outcomes. Alternatively, grants may be determined by unobserved factors also determining political outcomes. Our strategy will be to instrument being in power[196] in a municipality –deemed potentially endogenous- with cases of being in power after the formation of an anti-coalition and with cases of 1st ever being in power of a particular political party in a particular municipality. We define an anti-coalition as a coalition from which either the largest or 2nd largest party –in terms of vote and seat share- has been excluded, even though these 2 parties taken together have a majority of seats at their disposal so that these 2 parties could form a municipal coalition already by themselves.

We will operationalise politicisation as partisan –i.e. party-political- alignment. We will try to answer the question if municipalities that are party-politically aligned with the subsidising government level(s) receive more grants. The "partisan alignment hypothesis" differs from –and is more sophisticated than- the so called "standard partisan hypothesis". (see Migueis 2013 p. 2) The latter assumes that parties do not per se favour aligned lower governments with intergovernmental grants but that they simply favour lower government levels where they obtain *many votes*, whether those lower governments are aligned or not. The standard partisan hypothesis with respect to *intergovernmental* grants may rather be simplistic and naïve as trying to favour voters directly by means of intergovernmental grants may be *inefficient* for any given political party in power at the subsidising

[195] The Belgian Regions and Communities are the equivalent of the "states" in other federal countries. The 3 Belgian Regions (Flanders, Wallonia and Brussels) have geographic borders and are mainly in charge of infrastructure, economic policy, and local government. The 2 major Belgian Communities do not have clear borders as both the Flemish and the French Community are active within the –officially bilingual- Brussels Region. The Flemish and the French Communities are mainly in charge of education and care for the young and the elderly, resp. benefiting the Dutch-speaking and the French-speaking Belgians.

[196] By "being in power" we mean either being the largest party in power or being a junior partner within the governing coalition.

level if one or more *other* parties are in power within some of the lower level governments. With a view to getting re-elected as a subsidising government, the literature allows us to distinguish at least 3 reasons for the relative efficiency of trying to favour aligned lower level *governments* rather than sympathetic *voters*:

1) If intergovernmental grants are allocated to *un*aligned governments, the party in power at the lower level *instead of* the party in power at the higher level could *claim the credit* towards the favoured voters for the public goods provided thanks to the extra grants[197]. The party in power at the higher level would rather like to see *"its own"* local politicians claim the credit for the extra grants. This is because the former expects that the latter's obtaining extra grants would *signal* its high ability to voters, and hence increase the probability of the latter's re-election. In turn, re-election of a party at the lower level could reflect favourably on the party as a whole, including at the subsidising level. (Bracco e.a. 2012 p. 5) Indeed, there is empirical evidence that lower level governments benefit electorally from being aligned. Curto-Grau e.a. (2012) find that aligned municipal incumbents in Spain gain 10% more votes in municipal elections, which may be thanks to the fact that aligned municipalities receive more grants. Bracco e.a. (2014) find that aligned municipal incumbents in Italy have a 20 to 35% higher chance to get re-elected.
2) Baskaran and Hessami (2014 p. 7) hypothesise that the strength of a party at the lower government level would be one of the determinants of the strength of that party at the higher government level(s) for still another reason: the more lower level governments are aligned, the easier a party in power at the subsidising levels may be able *to implement its policies at the lower level(s)*. The less resistance to its policies a party governing at the higher level may experience, the more successful that party may be at the next elections at the subsidising level.
3) Brollo and Nannicini (2012) and Migueis (2013 p.2) put forward an explanation completely different from credit claiming and ability signalling towards *voters* in favour of partisan allocation: extra grants would be meant to increase the gratitude of *local politicians* towards their party. Hence the extra grants could help to *to ensure that local politicians will mobilise more voters* to support the party at the next elections *at the subsidising level*. Larcinese e.a. (2006 p. 453) give the example of US state governors who are aligned with the party of the President, and their presumed ability to "deliver" the vote of their state in presidential elections[198].

Remarkably, the existing literature allows us *also* to distinguish a reason for the relative efficiency of trying to favour *non-aligned* lower level governments rather than aligned lower level *governments.* In their study of the effect of partisan alignment on intergovernmental grants to municipalities in the German state of Hesse, Baskaran and Hessami (2014) find that rightwing state governments favour

[197] This is because, as Bracco e.a. (2012 p. 5) point out, an essential feature of *intergovernmental* grants is that they prevent the local voters to distinguish between the resp. lower level government's and higher level government's *contributions* to public good provision. Using the wording of Arulampalam e.a. (2009 p. 104): the grants receiving government *stands "between"* the grants providing government and the voters at the grants receiving government level. Or using the wording of Dasgupta e.a. (2004): when the electoral returns from spending are shared between the state government and the central government, then transferring funds to a government of the opponent party generates a "leakage" effect whereby the central government loses part of the electoral benefit from spending.

[198] However, Erikson e.a. (2014) find that a presidential candidate's vote share is *reduced* by 3 to 4 percentage points in states with a governor from the same party. They suggest as an explanation that voters may wish to *balance* their state government with the federal government. Their finding is similar to the one by Schelker (2011).

unaligned municipalities at the expense of aligned municipalities, i.e. they find a significantly *negative* effect of alignment with a rightwing state government on grants received. Their explanation is that in their panel only few municipalities are aligned with rightwing state governments, due to which these state governments would have been compelled to "buy off" the support of unaligned municipalities with more discretionary grants (possibly reserving other kinds of benefits than discretionary grants for aligned municipalities).

The remainder of the paper is structured as follows. First we motivate the municipalities of the Brussels Region of Belgium as an interesting sample for investigating the partisan grants allocation hypothesis. Next we give a description of our dependent variable, i.e. discretionary grants to Brussels municipalities, and of our independent variable of interest, i.e. partisan alignment of Brussels municipalities. Thereafter we situate our investigation of party-politically aligned allocation of grants to Brussels municipalities into the existing empirical literature. In the key part of our paper we then develop and test our empirical strategy, i.e. an IV estimation strategy based on partisan alignment as a result of the formation of an anti-coalition and as a result of 1st ever municipal coalition participation of Ecolo, one of our 4 major Brussels parties. In a final section we draw a number of conclusions and sum up remaining puzzles.

The main finding of our investigation will be that -rather surprisingly- municipalities of the Brussels Region that are party-politically aligned with a *smaller* and moreover *relatively new* party –Ecolo- in power at one or more of the Brussels municipalities' subsidising levels receive more discretionary grants than municipalities that are aligned with *larger* parties. However, these extra grants are not sufficient to compensate for the *decrease* in grants municipalities experience when they are governed by this same party Ecolo, if Ecolo is *not* also in power at one or more subsidising levels.

2. Motivation of the Brussels Region as a case study for the partisan allocation of intergovernmental grants

The partisan alignment hypothesis –as presented above- implicitly assumes that the subsidising levels rather than the subsidy receiving levels are the "acting" levels within "the game of aligned allocation of grants". However, of course the *subsidy receiving levels themselves*, rather than the subsidising governments could be the major player within the aligned allocation of grants, as they could be particularly able/motivated to lobby a subsidising government for extra grants in case they are aligned with this government. This is a possibility that seems to have been neglected in papers related to ours (Bracco e.a. (2012), Brollo and Nannicini (2012), and Curto-Grau e.a. (2012)). An exception is Migueis (2013 p. 20), who finds a partisan alignment effect on grants to Portuguese municipalities that is entirely attributable to one of the 2 major parties in Portugal, i.e. to the centre-right PSD and not to the centre-left PS. Migueis (2013 p. 20) hypothesises that his finding could be due to the larger power of PSD *local* politicians over PSD leadership compared to PS. Another exception is Sørensen (2003), who empirically explains part of the disparities in grants allocated to municipalities by the Norwegian central government with lobbying by municipal council members. Final exceptions are Khemani (2007 p. 707-708) and Biswas e.a. (2010), who analyse the lobbying of the Indian central government by the states.

The possibility that the *subsidy receiving levels themselves*, rather than the subsidising governments could be the major player within the aligned allocation of grants particularly seems to apply to the Brussels Region, as its municipalities and their mayors are reported to be powerful compared to the

Brussels Regional government. Over our sample period several ministers at the Regional level have apparently even preferred to give up their position to become mayor of a –in some cases even small- Brussels municipality[199]. In this respect, Cattoir e.a. (2009 p. 32) criticise discretionary grants to Brussels municipalities for the reason that in Brussels a relatively large number of municipal politicians are simultaneously a member of the parliament and/or of the government of the Brussels Region[200], thus increasing their influence over the allocation of discretionary grants to their own municipality. This is also due to the combination of Brussels' small size –consisting of 19 municipalities only- and it being a full-fledged Region of Belgium. The Brussels Region has competencies and institutions very similar to the 2 larger Regions of Belgium (Flanders and Wallonia). Some observers describe the level of the Brussels Region as a mere level for "dispensing cash" to the Brussels municipalities. Moreover, as opposed to e.g. Norwegian municipal politicians lobbying the central government as analysed by Sørensen (2003), Brussels municipal politicians are geographically very close to all their subsidising governments. Indeed, the Brussels Region hosts the governments of –evidently- the Brussels Region, and also of the Belgian Federation as well as of the French (and of the Flemish) Community.

Another reason why in a multi-level government setting such as the Brussels one the partisan alignment question seems particularly interesting is that the Brussels municipalities are not only subsidised by the Brussels Regional government and the French Community government, but also by the Federal government. Hence, Brussels municipalities can be fully aligned, partially aligned or fully non-aligned, depending on their alignment with 3, 2, one or none of the subsidising levels.

Moreover, while the above characteristic applies to *all* Belgian municipalities and not only to the Brussels ones, this paper focuses on the effect of partisan alignment on the intergovernmental grants to the 19 municipalities of the Brussels Capital Region only. With 1,119,088 inhabitants in 2011, the Brussels Region is the smallest of the 3 Regions of Belgium (the other 2 Regions being Flanders and Wallonia). We focus on the municipalities of the Brussels Region for the following reasons:

1) As illustrated by table 1 for the year 2009, the Brussels Region appears to be a Belgian Region in which municipalities receive a large amount of special purpose grants from higher-level governments as a share of total grants to municipalities. In line with Belfius (2010), we consider these special purpose grants (labelled "toelagen" by Belfius) as discretionary, i.e. distributed not according to a pre-agreed formula voted into a law. The Brussels Region's high share of special purpose grants implies that it scores rather low compared to other Belgian Regions as for the share of general purpose grants (labelled "fondsen" by Belfius) in the total budget[201]. These general purpose grants are considered formula-based by Belfius, and mainly comprise grants out of the resp. Regions' Municipality Funds[202] and grants distributed according to these Funds' criteria[203].

[199] A peculiar illustration of the power of the Brussels municipal level relative to the Brussels Regional level is the case of François-Xavier de Donnéa, a leading MR politician in the Brussels Region over our sample period and mayor of the City of Brussels from 1994 until 2000. Newspapers reported that de Donnéa was "consolated" with the post of Prime Minister of the Brussels Region after having lost the post of mayor of the City of Brussels due to an anti-coalition formed by PS in 2000. (see e.g. De Morgen 2000)

[200] This is also due to the combination of Brussels' small size –consisting of 19 municipalities only- and it being a full-fledged Region of Belgium. It has Regional –as well as municipal- competencies and institutions very similar to the 2 larger Regions of Belgium (Flanders and Wallonia).

[201] The rather low share of *total* grants in the *total budget* of Brussels municipalities is mainly to be explained by the high revenues out of municipal real estate taxes in the Brussels Region. The latter explain the high share of tax revenues in the total budget of the average Brussels municipality.

[202] In particular, grants out of the Brussels Region Municipality Fund are allocated according to a municipality's population size and density, surface, tax revenues, number of pupils, number of unemployed and number of

Table 1: Importance of formula-based and discretionary grants within municipal budgets, compared between Belgian Regions (2009)

	Total grants (% of budget)	Formula-based grants (% of total)	Discretionary grants (% of total)
Flemish Region	36	72	28
Walloon Region	41	59	41
Brussels Region	35	57	43

Source: Belfius (2010)

2) The Brussels Region as a whole (i.e. its municipalities and the Brussels Regional Government taken together) is the best funded region of Belgium on a per capita basis (considering grants and own tax revenues taken together). (see Algoed 2009) Moreover, the Brussels Region receives substantial extra grants from the Federal government following the fiscal federalism reform that was voted in 2013, amounting to an extra 461 million real euros in 2013 terms per year. As a consequence of this reform, the extent to which the Brussels Region is grants-funded will increase considerably.

3) The Brussels Region is a political battleground, i.e. for the 4 major political parties of the Brussels Region –MR, PS, CDH and Ecolo- several Brussels municipalities are "swing" municipalities. In several municipalities, being in power, once gained, is often expected to be at risk during the next municipal election, as has been illustrated by newspaper articles over our sample period (1995-2011) (see e.g. Le Vif/L'Express 2006), and as is shown by table 2 below. In particular the 2 smaller parties, CDH and Ecolo, have witnessed changes in being in power in on average about half of the 19 municipalities during each of the 2 municipal elections included in our sample period.

Table 2: Number of municipalities (out of a total of 19) in which being in power changed after the 2000 and the 2006 municipal elections, per individual party (4 major Brussels parties only)

	MR	PS	CDH	Ecolo	Total
2000	6	6	11	9	**32**
2006	8	6	6	11	**31**

Sources: Lentzen and Mabille (1994), Arcq e.a. (2001) and de Coorebyter (2007).

Municipalities of the Brussels Region have traditionally been dominated politically by MR. MR is the alliance that existed from 1992 until 2011 between the 2 Francophone Belgian parties PRL and FDF, including at the municipal level (though the name MR dates only from 2002)[204]. Within the Brussels Region, MR has over the last 20 years steadily lost electoral

poor. These criteria are confirmed annually by means of the voting of a law by the parliament of the Brussels Region. (see e.g. Belgisch Staatsblad 1998). These criteria are changed rather infrequently. The most recent changes to the criteria date from 1994 and 1998.

[203] Since the lowest level of disaggregation of our grants data is between special purpose and general purpose grants, we will simply equal special purpose grants to discretionary grants and general purpose grants to formula-based grants in what follows.

[204] Moreover, the 2 parties ran on 1 single list from the 1999 Federal, Regional and Community elections onwards, and –with some exceptions- from the 2006 municipal elections onwards. Where PRL and FDF ran on separate lists in the 1994, the 2000, and even the 2006 municipal elections, this was not necessarily to be

ground to PS, both in municipal and in Brussels Regional elections. Tables 3 and 4 illustrate the municipal case of growing power of PS relative to MR in terms of vote share and coalition participation resp. Both within the Brussels Region and in Francophone Belgium as a whole[205], PS and MR are the 2 largest parties[206]. Moreover, Brussels is the Region of Belgium where Ecolo, the Francophone Belgian Green party and one of the largest Green parties of Europe, is the strongest. Finally, CDH, which was until 2002 known as PSC[207], and which is the Francophone Belgian party that has been most often in power at the various subsidising levels during Belgian history, has had to defend its –rather limited- power base within Brussels municipalities over our sample period. In spite of the steady decline of the income per capita position of the Brussels Region compared to the 2 other regions of Belgium over our sample period[208], parties covet being in power in the capital region of Belgium, as this brings extra visibility at the Federal level.

Table 3: Average vote shares of the 4 largest political parties (%) in the Brussels municipalities in the 1994, 2000, and 2006 municipal elections

Election	MRmunvote	PSmunvote	CDHmunvote	ECOLOmunvote
1994	34.2	18.8	14.6	8.4
2000	35.6	20.6	11.1	17.7
2006	34.7	25.4	13.1	13.3
Total average	**34.8**	**21.6**	**12.9**	**13.1**

Sources: http://www.ibzdgip.fgov.be/result/nl/main.html, http://www.bisa.irisnet.be/themas/verkiezingen#.UqXcgDsjJsQ, own calculations.

interpreted as if the alliance did not function, as in several occasions running on 2 separate lists had to serve a tactical purpose. In most occasions over our sample period where both PRL and FDF won seats, PRL and FDF were either both part of a municipal government or were both in opposition.
Also earlier in Belgian political history, PRL and FDF had formed alliances.

[205] By Francophone Belgium we mean the Walloon and the Brussels Region, though the Brussels Region is administratively bilingual.

[206] Political parties in Belgium are split along linguistic and therefore largely along geographic lines. No single party in Belgium wins seats both in the Dutch-speaking Flemish Region and in Francophone Belgium. Exceptions are the few seats Flemish parties have (until 2010) won in the Brussels Region and the few seats Francophone parties have (until 2010) won in Halle-Vilvoorde. Halle-Vilvoorde is the Flemish part of the Federal electoral district Brussel-Halle-Vilvoorde. Brussel-Halle-Vilvoorde is the only Federal electoral district that has ever straddled 2 Regions, i.e. until and including the elections of 2010, after which the district was split into Brussels and Halle-Vilvoorde.

[207] For the sake of simplicity we will refer to PSC with CDH also with respect to the period before PSC changed its name into CDH.

[208] In real 2011 terms, income per capita in the Brussels Region only increased from 12,459 to 13,612 euros over our 1995-2011 sample period.

Table 4: Number of Brussels municipal coalition participations of the 4 largest political parties in the Brussels municipalities, after the 1994, 2000, and 2006 municipal elections (out of a maximum of 57)[209]

Election	MR	PS	CDH	Ecolo
1994	16	12	9	2
2000	14	11	7	11
2006	15	13	9	6
Total	**45**	**36**	**25**	**19**
Maximum	57	57	57	57

Sources: Arcq e.a. (2001) and de Coorebyter (2007).

Table 5 provides the full names of the 4 major Brussels parties and situates them on a left-right axis. The difference in ideology between the 4 parties can be considered relatively small, with even MR having a more leftwing profile than e.g. its Flemish liberal-conservative counterpart.

Table 5: Full name and ideological position of the 4 major Brussels parties

Acronym	Full name	Ideology
PS	Parti Socialiste	Left
Ecolo	Ecolo	Left
CDH	Centre Démocrate Humaniste	Centre-left
MR	Mouvement Réformateur	Centre-right

4) There also exist some anecdotes concerning the Brussels municipalities that are suggestive of party-political steering of particular discretionary grants categories comprised in our data. An example for *the Brussels Regional level* are the "contrats de quartier" (urban renewal contracts), a municipal subsidy category established in 1994, cumulatively amounting to 408 million nominal euros for all beneficiary municipalities taken together over our sample period. In 2012 a leading Francophone Belgian weekly, "Le Vif/L'Express" (2012), reported extensively on a complaint of an MR member of the Brussels Regional parliament and councillor in the municipality of Ukkel that since the establishment of the Brussels Region in 1989[210] the distribution of several grants categories to Brussels municipalities would have become skewed against the –traditionally MR dominated- south-eastern municipalities. These mainly include some prominent special purpose subsidies allocated by the Brussels Regional government, such as the "contrats de quartier". Anecdotal evidence of party-political steering of particular discretionary grants categories comprised in our data also exists at *the federal level*, i.e. with respect to the federal Fund for Large Cities. We have not found any anecdotes suggestive of possible party-politically aligned allocation of the grants of *the French Community level* to Brussels municipalities. Our data suggest that these concern mostly –large- education and childcare grants. Strikingly, when conducting our below regression analysis while leaving the French Community level out of the composition of our partisan

[209] For MR the number is 16 from 2010 onwards, as it entered the coalition in Jette halfway the legislature. For PS the number is 12 from 2010 onwards, as it left the coalition in Jette halfway the legislature.

[210] Since its establishment in 1989 the main political party in power at the level of the Brussels Regional government has always been PS, with the exception of MR from 1999 until 2004.

alignment variables of interest, our regression results stay the same. Regression results *do* change if we leave out the Federal level or the Brussels Regional level[211].

5) We have not found data readily available on the party-political composition of all Belgian municipal governments over our sample period. In cases in which such data do exist, the party-political affiliation of several parties governing at the municipal level is not clear as several of these parties take a local name that hides their affiliation with a party that is politically represented at one or more of the governments that subsidise Belgian municipalities. As the Brussels Region consists of only 19 municipalities, the several publications of CRISP (Lentzen and Mabille (1994), Arcq e.a. (2001) and de Coorebyter (2007)) enabled us to determine the party-political composition of the Brussels municipal governments over our sample period, and hence their partisan alignment status.

3. The dependent variable: discretionary grants to Brussels municipalities

Discretionary grants to Brussels municipalities is our dependent variable. We focus on discretionary special purpose grants rather than on formula-based general purpose grants, as the empirical political economics literature demonstrates that the former are more prone to steering for (party-)political reasons. Our sample period is the 1995-2011 period, which are the years for which we have obtained data on grants to Brussels municipalities. The source of our grants data is Belfius, formerly known as Dexia, which is a bank specialising in lending to local governments[212].

Table 6 shows discretionary grants per capita received by individual municipal governments. The considerable variation both *between municipalities* –as is clear from comparing the means- and *over our sample period* –as shown by the standard deviations and by the coefficients of variation[213]- is striking. However, standard deviations and coefficients of variation are much higher for some municipalities than for other. Note that the municipality receiving on average the most grants per capita is the City of Brussels, which is the largest of the 19 municipalities of the Brussels Region.

Unfortunately the Belfius data do not enable us –nor for municipalities of the Brussels Region nor for Belgian municipalities in general- to distinguish between total discretionary grants *per individual subsidising government*. We hence have no information of the relative magnitudes of Federal, Regional, resp. Community government subsidies to Brussels municipalities. As in Belgium ”oversight of and support to municipalities” as an expenditure responsibility was largely decentralised to the Regions in 1988, we would expect Federal subsidies to municipalities to be rather limited compared to Regional subsidies over our sample period. This aggregate nature of our grants data makes the fact that we consider grants provided by 3 different levels of government less innovative compared to the existing literature, as this aggregate nature makes it harder to find out which kind of partisan alignment –Federal, Regional, or Community government alignment- potentially drives grants allocation to municipalities.

[211] The regression results of these changes to our sample are not reported below.
[212] Belgian municipalities report about their finances to Belfius on a voluntary basis. Our requests for data directed at the Brussels Regional government and at the individual Brussels municipalities have not generated more detailed grant data than those kindly provided by Belfius.
[213] The coefficient of variation equals the standard deviation divided by the mean.

Table 6: Summary statistics of discretionary grants (in euro per capita) received by individual municipal governments

Municipality	Mean	Standard deviation	Coefficient of variation
Anderlecht	214.9	65.0	0.30
Brussel	437.1	75.2	0.17
Elsene	229.7	71.5	0.31
Etterbeek	259.7	64.5	0.25
Evere	264.6	77.9	0.29
Ganshoren	109.5	36.1	0.33
Jette	210.5	82.4	0.39
Koekelberg	194.1	38.2	0.20
Molenbeek	203.9	44.6	0.22
Oudergem	125.9	49.2	0.39
Schaarbeek	190.2	50.5	0.27
Sint-Agatha-Berchem	150.0	43.3	0.29
Sint-Gillis	317.4	106.4	0.34
Sint-Joost-ten-Node	394.8	108.6	0.28
Sint-Lambrechts-Woluwe	107.7	18.8	0.17
Sint-Pieters-Woluwe	175.4	40.6	0.23
Ukkel	112.6	37.2	0.33
Vorst	278.6	68.8	0.25
Watermaal-Bosvoorde	163.2	48.4	0.30

Source: Belfius

4. The independent variable of interest: alignment of Brussels municipalities with respect to individual political parties

We limit our analysis to the partisan alignment effect with respect to the 4 major parties active at the level of the Brussels municipalities –which are all Francophone-: MR, PS, CDH, and Ecolo. Together these parties obtained on average 82.5 % of the total vote in the Brussels municipal elections of 1994, 2000, and 2006, with MR obtaining on average 34.8% of the vote, PS 21.6%, CDH 12.9% and Ecolo 13.1% (see table 3 above). Part of the coalition participations of these 4 largest parties were however a consequence of having campaigned on a joint list with another major Francophone party, except for Ecolo, which always campaigned on a separate list over our sample period[214].

The remainder of the municipal vote was obtained by local Brussels parties, far right parties –fully excluded from power over our entire sample period-, and the 5 Flemish parties that both participated in some of the municipal coalitions and in some of the coalitions at the subsidising levels over our sample period. These residual vote shares are in general small, as well as decreasing over our sample period. The same holds for the number of separate lists submitted by Flemish parties. E.g. in the 2006 municipal elections those Flemish parties that submitted a separate list, together obtained less than 10% of all votes. (de Coorebyter 2007 p. 20) When we also include the 5 Flemish parties that both

[214] Our regression results below are very similar if we do not count as coalition participations those by junior coalition partners that *ran on a joint list* with a senior coalition partner.

participated in some of the municipal coalitions and in some of the coalitions at the subsidising levels into the regression analysis reported below[215], none of the 5 extra coefficients of interest is significant, and their inclusion does not appear to change the size and the significance of the coefficients of interest for the Francophone parties.

For the sake of simplicity, we will use the coalition compositions of the outgoing municipal government in the municipal election years of our sample period. Municipal elections always took place in October over our sample period. Likewise, for the sake of simplicity we will also use the coalition composition of the outgoing government in the election years of our sample period at the subsidising levels. At the subsidising levels, elections always took place in May or June over our sample period. Using the coalition composition of the outgoing government in election years has the following additional advantages:

1) it takes into account the possibility of a political "business cycle", i.e. the possibility that parties in power may concentrate their aligned allocation of grants right before an election
2) it takes into account that subsidising levels decide on their budgets –including discretionary grants- for year t already in year t-1 (and more generally, that subsidising levels may need some "lag time" to make discretionary grants arrive at the intended municipal level)

Table 7 shows the number of simultaneous participations of each of the 4 major Brussels parties in coalitions at the various subsidising levels (i.e. at the Federal, Regional, and Community government levels). Changes in these numbers were caused by the various Federal, Regional, and Community elections over the 1995-2011 period.

Table 7: Number of simultaneous coalition participations of the 4 major Brussels parties at the subsidising levels after the various Federal and Regional elections over our sample period

Election	PS	MR	CDH	Ecolo
1995 Federal, Regional, and Community elections	**3**	**1**	**2**	**0**
1999 Federal, Regional, and Community elections	**3**	**3**	**0**	**1**
2003 Federal elections	**3**	**3**	**0**	**0**
2004 Regional and Community elections	**3**	**1**	**2**	**1**
2007 Federal elections	**3**	**1**	**3**	**1**
2009 Regional and Community elections	**3**	**1**	**3**	**2**
2010 Federal elections	**3**	**1**	**3**	**2**
Average per single year	**3**	**1.6**	**1.7**	**0.8**

Source: Websites of the Federal, Brussels Regional, and French Community parliaments

Table 7 shows that PS was in power at every subsidising level over our entire sample period (as a matter of fact ever since 1988). From the bottom row of table 7 it appears that overall power participation of CDH at subsidising levels has on average been slightly higher than that of MR, with Ecolo showing the lowest power participation.

Our independent variables of interest then are the interaction variables "MRmungovt*subsgovt" for MR, "CDHmungovt*subsgovt" for CDH, "ECOLOmungovt*subsgovt" for Ecolo, and "PSmungovt*3" for PS. The resp. "mungovt" variables are dummies taking value 1 if the party in question is part the municipal coalition. The resp. "subsgovt" variables are count variables -counting in how many subsidising governments the party simultaneously participated-, hence taking a value

[215] Regression results of this inclusion are not shown below.

between 0 and 3[216]. Taking MR as an example: while after the 2004 Regional and Community elections MR kept on being part of the Federal governing coalition, it was excluded from the Brussel Regional governing coalition as well as from the French Community governing coalition. Therefore between 2004 and 2005 the "MRsubsgovt" count variable decreases from 3 to 1 (see table 7).

Our interaction variable in question hence measures per individual party the *degree* of partisan alignment between a municipality and the subsidising government levels. Construction of the alignment variable as a count variable rather than as a dummy is a novelty within the literature on partisan alignment (see e.g. Bracco e.a. (2012), Brollo and Nannicini (2012), Curto-Grau e.a. (2012), and Migueis (2013)). There is no need to interact "PSmungovt" with a count variable measuring the number of subsidising governments that PS is taking part in, because PS was over our entire sample period simultaneously in power at every subsidising government. Therefore we simply multiply our original "PSmungovt" dummy by 3, so as to make regression coefficients comparable between parties. Hence our PS interaction variable only takes values 0 and 3.

Graph 1 below shows descriptive statistics for our 4 constructed interaction variables of interest, i.e. for their *means* over time. Obviously, these means take maximum values of 3 and minimum values of 0. Years in which these alignment variables are able to change are years following an election year. Election years are indicated with a vertical bar. As expected, average alignment is highest for PS (because of its high coalition participation at the subsidising level), 2nd highest for MR (because of its high coalition participation at the municipal level), 3rd highest for CDH and lowest for Ecolo.

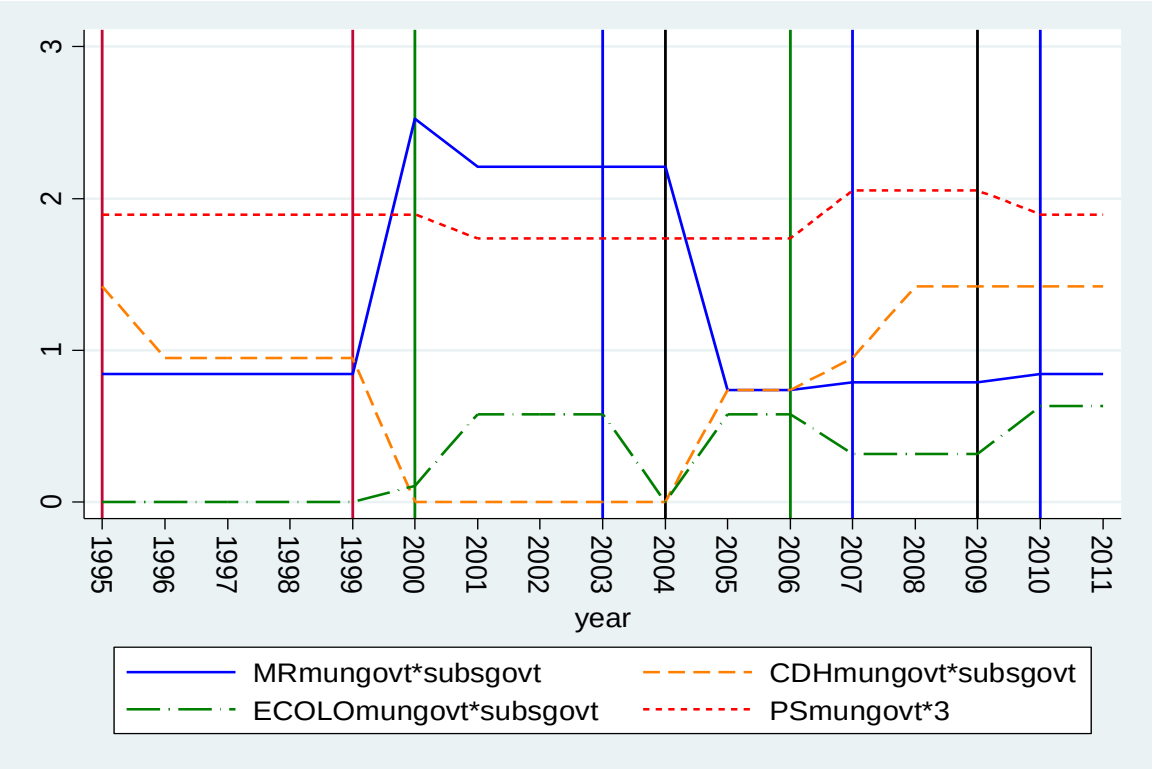
We also note considerable variation *over time* in mean alignment for each individual party, except for PS[217]. Unsurprisingly, the years after the 2 municipal elections show large overall changes in mean alignment. This also holds for the year 2000, as it is the year after the concurrent Federal, Community, and Regional elections of 1999. As a result of these elections PS exchanged CDH for MR and Ecolo as coalition partners in the Federal government and CDH for MR in the Brussels Regional government. Also the year 2005 -the year after the Community and Regional elections of 2004- shows large changes. After these elections, PS exchanged MR for CDH and Ecolo in the Brussels Regional government, and MR for CDH in the French Community government.

Note that –for reasons of simplicity- graph 1 shows *mean* alignment per party, even though our interest lies in the alignment of *individual municipalities* per party. Many municipalities experience more variation over time in their alignment than that the variation of the mean shows.

[216] As we do not know the resp. shares of the 3 subsidising levels in total discretionary grants, we simply increase our count variable by 1 for each extra subsidising level at which a particular party is in power.

[217] This rather stable mean PS alignment is due to its constant coalition participation at the subsidising levels. In contrast, there has been considerable variation in PS coalition participation at the municipal level over our sample period.

Graph 1: Means of partisan alignment of Brussels municipalities for the 4 largest political parties



Sources: Wikipedia, Arcq e.a. (2001) and de Coorebyter (2007)

Legend of graph 1: Red vertical bars: 1995, 1999: concurrent Federal, Regional and Community elections

Green vertical bars: 2000, 2006: municipal elections only

Blue vertical bars: 2003, 2007, 2010: Federal elections only

Black vertical bars: 2004, 2009: concurrent Regional and Community elections

After the construction of our interaction variables "MRmungovt*subsgovt", "CDHmungovt*subsgovt", "ECOLOmungovt*subsgovt", and "PSmungovt*3" we can easily check how grants are distributed over Brussels municipalities according to alignment status. Table 8 shows that the more aligned an Ecolo and a PS municipality is, the more grants it obtains. Strangely, the reverse holds for MR, while there is no clear pattern for CDH. However, the differences in grants obtained according to alignment status are not significant.

Table 8: Summary statistics of discretionary grants distribution (in euro per capita) according to alignment of Brussels municipalities with the 4 largest Brussels political parties

Alignment variable	Alignment value	Mean	Standard deviation	Observations
MRmungovt*subsgovt	0	251.8	127.7	77
MRmungovt*subsgovt	1	212.7	94.2	170
MRmungovt*subsgovt	2	.	.	0
MRmungovt*subsgovt	3	188.5	106.2	71
CDHmungovt*subsgovt	0	214.0	111.0	216
CDHmungovt*subsgovt	1	.	.	0
CDHmungovt*subsgovt	2	201.6	102.5	57
CDHmungovt*subsgovt	3	249.3	93.2	45
ECOLOmungovt*subsgovt	**0**	**213.1**	108.1	231
ECOLOmungovt*subsgovt	**1**	**222.4**	113.3	75
ECOLOmungovt*subsgovt	**2**	**251.1**	51.0	12
ECOLOmungovt*subsgovt	3	.	.	0
PSmungovt*3	**0**	**169.4**	66.3	121
PSmungovt*3	**3**	**245.9**	117.7	197
Total		**216.7**	**107.8**	**318**

Source: own calculations

5. Situating our study within the literature

5.1. Methodologically situating our study within the literature

In what follows we will investigate if the fact of a Brussels municipality becoming better aligned politically with one of the subsidising levels, brings more grants to this Brussels municipality. Hence our study is related to the many studies that have found that the distribution of grants to lower-level governments is determined by the latter's partisan alignment status, such as Larcinese e.a. (2006) for US states; Solé-Ollé and Sorribas-Navarro (2008) for Spanish municipalities; Veiga and Veiga (2013) for Portuguese municipalities; Rodden and Wilkinson (2004), Khemani (2007), Arulampalam e.a. (2009), and Biswas e.a. (2010) for Indian states; and Rodden (2005 p. 181-221) for German states. Rodden (2005) and Khemani (2007) investigate the effect of partisan alignment on states' *budget deficits* rather than on *grants* received, but find a –positive- correlation between deficits and extra grants received.

However, none of the abovementioned –"1st generation"- studies of partisan alignment takes into account possible endogeneity of being in power. Partisan alignment –and more in particular a party's

being in power at a lower government level as a pre-condition for partisan alignment[218]- could be endogenous to grants received. Being in power at a lower government level could e.g. be fostered by past grants received –or even by a *lack* of past grants received, as argued by Fiva and Halse (2014 p. 12)-, e.g. via an increased share of the vote. Also, there could be –observable or unobservable- time-variant determinants *both* of municipal coalition participation of a particular party *and* of grants received that have been omitted from our regression specification[219]. Therefore, our study is even more related to the increasing number of –"2nd generation"- papers that attempt to control for endogeneity of being in power by means of using a Regression Discontinuity Design (RDD) with respect to election results and being in power as independent variables (see e.g. Bracco e.a. (2012), Brollo and Nannicini (2012), Curto-Grau e.a. (2012), Migueis (2013), and Baskaran and Hessami (2014)). This is because also in our study we will make an attempt to find a way around the problem of the endogeneity of being in power.

Curto-Grau e.a. (2012) refine the abovementioned Solé-Ollé and Sorribas-Navarro (2008) paper by focusing on vote margin within an RDD. Using a "fuzzy" RDD they find an even stronger positive effect of party-political alignment on grants received by municipal governments than in their 2008 study, which uses a Fixed Effects approach. Curto-Grau e.a. (2012) define their vote margin variable as the vote share that the block of the incumbent party at the subsidising level has to win (lose) to obtain (lose) the majority of the votes at the municipal level. This approach is suited to the political situation of Spain as investigated by Curto-Grau e.a. (2012), because over their sample period only 30% of Spanish municipal governments were coalition governments, which moreover were coalitions along the ideological left-right axis.

The Portuguese situation seems even more straightforward than the Spanish one. The Portuguese blocks constructed by Migueis are a block dominated by the centre-right PSD and a block dominated by the centre-left PS. Towards the end of Migueis' sample period, the governments of 269 out of 305 Portuguese municipalities were either dominated by PSD or by PS. PSD and PS appear never to be in a coalition together, neither at the central government level, nor at the municipal level. In contrast to Spain moreover, the Portuguese party winning the most votes, always gains power, so Migueis is able to make use of the "sharp" RDD, which is more straightforward than the "fuzzy" RDD.

With respect to results, Curto-Grau e.a. (2012) find that newly aligned municipalities that have been won by a narrow margin are particularly *rewarded* with more grants, while previously aligned municipalities that have been lost by a narrow margin are particularly *punished* with less grants. Similarly using an RDD for Brazil, Brollo and Nannicini (2012) come to the same finding with respect to less grants to narrowly lost aligned municipalities. Migueis (2013) comes to the same finding with respect to grants to aligned and non-aligned municipalities in Portugal, but irrespective of the vote margin. The paper by Bracco e.a. (2012) on Italian municipalities mostly resembles the one by Brollo and Nannicini (2012) as for research question and empirical strategy. Bracco e.a. (2012) not only find a positive effect of alignment on grants received, but also a negative effect on local tax revenue. Similarly to the studies on Spain and Portugal, both the study on Brazil and the study on Italy manage to make use of an RDD as both manage to assign each individual party to either a leftwing or a rightwing block.

[218] Evidently, for a municipality to be aligned with a particular political party in power at one of the subsidising levels, this particular political party needs to be in power at the level of the municipality.

[219] Moreover, Folke (2013 p. 7) explains that the direction of the –downward or upward- endogeneity bias in OLS regressions of a policy outcome variable on a political variable is hard to predict, because of the complexity of the entanglement of political variables and factors correlated with these variables, while both types of variables may have policy effects.

Because the papers by Bracco e.a. (2012), Brollo and Nannicini (2012), Curto-Grau e.a. (2012), and Migueis (2013), as well as the already earlier discussed paper by Baskaran and Hessami (2014), all explain grants to municipalities by partisan alignment while making use of an RDD to find a way around the problem of the endogeneity of alignment, these papers are *from a methodological viewpoint* the ones that are most closely related to our paper.

5.2. Situating our study within the literature from a contents perspective

From a contents-wise viewpoint, the studies by Rodden and Wilkinson (2004) and Khemani (2007) are most similar to ours. This is because Rodden and Wilkinson (2004) and Khemani (2007) investigate for the case of India the partisan alignment effect *per individual party*, i.e. distinguishing between *large and small* coalition members. Rodden and Wilkinson (2004) find that Indian parties that are small but pivotal at the central level –i.e. parties that are *needed* in every coalition that commands the support of a majority of seats- obtain more grants for their "home state" in case of partisan alignment than the 2 dominant parties in India (Congress and BJP) over their sample period. Together with Arulampalam e.a. (2009 p. 117), Rodden and Wilkinson (2004 p. 10 and 17) suggest that small Indian parties in power at the central level not only secure more grants to their "home state", but also more public sector appointments, subsidised borrowing and regulation favouring those small parties. However, in contrast to Rodden and Wilkinson (2004)'s finding, Khemani (2007) finds that it is partisan alignment of an Indian state with the *dominant* party in power –and not with junior coalition members- at the central level that is associated with higher deficits, and hence with the extra grants that the latter manage to generate.

These findings with respect to the association between party size and extra grants secured thanks to partisan alignment relate to a tradition in political science investigating the effect of party size on resources secured in general –i.e. not only on the amount of grants- within a context of coalition formation bargaining –i.e. not necessarily in a context of partisan alignment. More in particular, Ansolabehere e.a. (2005) find empirically that it is mostly the *larger* coalition party, i.e. the formateur party, rather than the *smaller* coalition party that is able to secure disproportionally more resources[220]. Ansolabehere e.a. (2005) hence find empirical support for the well-known legislative bargaining framework developed by Baron and Ferejohn (1989), in which the formateur is able to obtain more resources thanks to his proposal power.

However, the findings of Ansolabehere e.a. (2005) seem to contradict those of another related political science literature, which studies the consequences of intra-coalition power relations in particular for the distribution of *ministerial posts*, such as Browne and Franklin (1973), Schofield and Laver (1985), and Laver and Shepsle (2000). Browne and Franklin (1973) and Schofield and Laver (1985) find empirical evidence that sheer smallness of coalition parties increases their "spoils of office", in particular their ministerial posts in central governments, at the expense of large coalition parties. Browne and Franklin (1973) find that large parties on average contribute 6% more parliamentary or council seats to a coalition that that they receive ministerial posts. Browne and Franklin (1973 p. 461) suggest that smallness may provide extra payoffs not because small coalition partners would be more

[220] Relatedly, there exists a large literature finding that rather than dominant political parties, dominant *individual politicians* manage to secure more resources, in particular more grants for their electoral district. Examples are Milligan and Smart (2005) and Jennes and Persyn (2015) with respect to individual ministers, Golden and Picci (2008) with respect to powerful politicians within Italian political parties, and Knight (2005 and 2008) with respect to powerful members of the US Congress.

likely to be pivotal[221], but because they are less likely than large coalition partners to be able to inhibit the activity of the dominant coalition party. Complementarily, because the major coalition party often controls more than the absolute majority of all ministerial postings as well as public policy itself[222], it could well afford to surrender one or 2 "bonus" ministerial postings. However, another way of looking at the above is that the lack of control over public policy that a junior coalition partner may experience could provide it with an extra motivation to bargain for extra ministerial postings as a compensation (Browne and Franklin 1973 p. 461).

Conversely, a larger party may derive other –perhaps more important but less visible- benefits than more postings or extra discretionary grants from holding office, such as a more exclusive control over public policy in general. This possibility reminds of the statement by Folke (2013 p. 4) –made within the context of Swedish municipal coalition politics- that "larger parties focusing on primary policies {such as taxing and spending} could win the support of small parties by supporting them on secondary policies {such as environment and immigration}". At the same time, Folke (2013 p. 31) admits that such trading of support is hard to measure.

The political economics literature points at yet another reason why small parties may be associated more strongly than large parties with the partisan allocation of grants. It is the uncertainty argument of Persson and Tabellini (2000 p. 345-351): the more a governing party is *uncertain* that it will still be in power after the next elections, the more it will overspend in the current legislature by means of running a budget deficit. Applied to the subject of the partisan alignment effect on grants, we could hypothesise that governing parties at the *subsidising* level that are less certain of remaining in power will try to increase spending in terms of increasing the allocation of grants to friendly governments, and that governing parties at the *municipal* level that are less certain of remaining in power will try to increase spending in terms of lobbying for an increase in grants allocated by friendly governments[223].

6. IV regression as an alternative empirical strategy to an RDD

There seem to be a number of major handicaps that the context of our study -the political system of the Brussels municipalities- poses to the use of an RDD, compared to the context of the related studies overviewed above -the political systems of the Brazilian, Italian, Portuguese, and Spanish municipalities.

A first handicap is our limited sample size (312 observations = 19 municipalities * 17 years, minus 11 observations missing due to missing financial control data for a number of municipalities and years). It

[221] This is in contrast to what Rodden and Wilkinson (2004) find for India. Moreover, Browne and Franklin (1973 p. 461) –as well as Folke (2013 p. 4)- correctly argue that a *large* party has a higher probability to be pivotal –i.e. indispensable for coalition formation- than a small party, and more generally that the coalition formation power of a large party is on average larger than the coalition formation power of a small party. (see below)

[222] Both the dominant coalition party and the junior coalition party may hence obtain "spoils of office". Only, their resp. advantages of holding office may be of a different nature. The findings by Browne and Franklin (1973 p. 461) therefore do not necessarily contradict those of Ansolabehere e.a. (2005).

[223] Pettersson-Lidbom (2001) tests the theoretical finding of Persson and Tabellini (2000) for Swedish municipalities. He finds mixed support for the hypothesis of Persson and Tabellini (2000), as he finds that a rightwing party that is certain of being voted out of a Swedish municipal government *increases* debt with 15% compared to when it is certain of being voted again into government, but that –even more remarkably- a leftwing party *decreases* debt with 11% in such a situation.

implies that there are few observations around any "being in power" kink/threshold, while an RDD particularly makes use of these observations.

A 2nd handicap is that assigning parties active at the Brussels municipality level to either a left-wing or a right-wing block with a view to verifying which block took the hurdle of the 50% seat share threshold -as Bracco e.a., Brollo and Nannicini, Curto-Grau e.a., and Migueis did- seems impossible. Over our sample period we did not find any preference pattern for ideological composition of municipal coalitions in the Brussels Region, as demonstrated by the behaviour of the 4 largest political parties at the Brussels municipal level over our sample period[224].

This should be obvious from table 9 below. It shows the number of municipalities in which a particular party was selected as a junior coalition partner by the major coalition party, as a share of total municipalities in which the latter was the major coalition party[225]. Table 9 shows e.g. that although MR is usually thought of as rather right-wing and PS and Ecolo are usually thought of as rather left-wing (see also table 5 above), e.g. MR-PS and MR-Ecolo coalitions are rather frequent. In general, it shows the absence of coalition preferences between the 4 major Brussels parties.

Table 9: Number of municipalities in which a particular party was selected as a junior coalition partner by the major coalition party (as a share of municipalities dominated by the latter)

Major party	**MR**			**PS**			**CDH**		
Junior partner	**PS**	**CDH**	**Ecolo**	**MR**	**CDH**	**Ecolo**	**MR**	**PS**	**Ecolo**
1994	6/10	3/10	1/10	4/5	1/5	0/5	1/3	1/3	0/3
2000	4/11	1/11	4/11	2/5	3/5	3/5	1/2	1/2	2/2
2006	4/9	3/9	4/9	3/8	3/8	1/8	2/2	1/2	1/2

Sources: Lentzen, E. and X. Mabille (1994), Arcq e.a. (2001) and de Coorebyter (2007)

Because of the limitations of analysing partisan alignment of grants to Brussels municipalities with an RDD, we develop an alternative empirical strategy. It consists of an IV regression set-up with alignment after the formation of an anti-coalition and after 1st ever municipal coalition participation as IVs for overall alignment[226].

6.1. IV for MR, PS, and CDH alignment: alignment after the formation of an anti-coalition

Rather than to find a way around the problem of endogeneity of alignment by means of an RDD, we resort to a -more traditional- IV regression set-up. The instrument we will use is the coming to power –and the expulsion from power- of a political party within a Brussels municipality *after the formation*

[224] We would not claim that such lack of preferences would be typical for the Brussels municipalities in Belgium. Ideology-based coalition preferences also became weaker at the subsidising levels over our sample period. This is in contrast to most of the preceding 20th century, when there were hardly any coalitions of which both the socialists and the liberals were a member, with both of them instead preferring the Christian-democrats as a coalition partner, while the Greens participated for the 1st time in a coalition at the subsidising level in 1999.

[225] For the major party, the sum of all shares may be larger than 1 because in several municipalities coalitions consisted of more than 2 parties.

[226] Hence we use mere *subsets* of the original and possibly endogenous independent variables as the IVs. This is an IV strategy similar to the one implemented by Dahlberg et al. (2008) when investigating the flypaper effect for Swedish municipalities. They use a *census-determined* (i.e. partial) change in grants to instrument their endogenous independent variable, i.e. the *actual* (i.e. total) change in grants received by Swedish municipalities.

of an anti-coalition. We will use the latter as an instrument for the coming to power of a political party in a Brussels municipality in general.

We argue that (being prevented from) coming to power by means of an anti-coalition is rather a "coincidence", i.e. not related to past grants received by the municipality in question. In other words: grants received by municipalities after the formation of an anti-coalition *by a particular party* should and can be compared to grants received by (other) municipalities after the formation of an anti-coalition *from which that same party was excluded, and* the difference between them can be attributed to being in power.

Table 10 gives an overview of all anti-coalitions we identify over our sample period. The vote shares of the largest party participating in the anti-coalition –labelled the "perpetrator"- as well as of the largest party that was excluded from the anti-coalition –labelled the "victim"- are indicated in columns (3) till (6), with the vote share of the "perpetrator" of the anti-coalition indicated in bold. We characterise 8 coalitions as anti-coalitions, out of a total of 57 coalitions (= 19 municipalities * 3 municipal election years).

Compared to column (7) "Motivation/effect" of table 10, we provide the following more quantitative definition of an anti-coalition: an anti-coalition is a coalition from which either the largest or 2nd largest party –in terms of vote and seat share- has been excluded, *even though these 2 parties taken together have a majority of seats at their disposal so that these 2 parties could have formed a municipal coalition already by themselves*. The latter should be clear from the final column of table 10. As a result of the formation of the anti-coalition the governing coalition consists of more parties –i.e. on average of 3.3 parties as shown by column (8) of table 10- compared to the case in which the 2nd largest party would not have been excluded –i.e. 2 parties[227]. As a consequence the party that "falls victim" to an anti-coalition is *large* in terms of vote share. Table 10 shows that the smallest "victim" of an anti-coalition obtained 23% of the vote[228].

We argue that anti-coalitions are "coincidence", i.e. hard to foresee or anticipate. We assume them to happen rather unexpectedly to a traditionally dominant party, possibly *in spite of* having secured extra grants to its municipality in the legislature preceding the election after which the anti-coalition was formed. We also assume them to happen rather unexpectedly to the party that –instead of the excluded party- becomes the dominant coalition party thanks to assembling the anti-coalition, because an anti-coalition seems rather hard to assemble, as it typically requires the agreement of more parties than the average municipal coalition (see column (8) of table 10). As a consequence, we assume that before the elections in question the traditionally dominant party did not anticipate that it would become excluded from the coalition and that before the elections in question the newly dominant party did not anticipate that it would become the newly dominant party of the coalition.

[227] The exception is the anti-MR coalition in Vorst, which consisted only of 2 parties, but also this coalition has far less seats at its disposal –only 1 seat more than required for a majority- than a coalition between the 2 largest parties in terms of seats would have had.

[228] When comparing this share to the average vote shares of the 4 major Brussels parties over our sample period, we notice that only the average vote share of MR is higher than 23%.

Table 10: Anti-coalitions formed after the 1994, 2000, and 2006 municipal elections[229]

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
election	**municipality**	**MR (%)**	**PS (%)**	**CDH (%)**	**Ecolo (%)**	**Motivation / effect**	**Coalition parties (#)**	**Coalition seats (%)**	**Seats "perpetrator" + "victim" (%)**
1994	**Brussel**	**28**		23		Dispose of Demaret + Vanden Boeynants (CDH)	4	62	74
1994	**Jette**	24		**28**		Continue CDH led "progressive" coalition, despite Thys' forced resignation as Brussels Regional minister	4	58	64
2000	**Anderlecht**	**30**	28			Break with 50 years' PS dominance	3	53	67
2000	**Brussel**	28	**23**			Revenge for Anderlecht anti-PS coalition formed in 2000	4	57	62
2000	**Elsene**	40			**29**	Wish of local PS and CDH to break with past and to form “progressive” coalition with Ecolo	3	51	78
2000	**Ganshoren**	**33**		29		Break with 40 years' CDH dominance + Beauthier mayorship, after his death (1999)	3	64	68
2000	**Jette**	23		**27**		Continue CDH led "progressive" coalition, despite death of Thys, 1977-99 mayor	3	64	61
2006	**Vorst**	40	**31**			Revenge for exclusion of PS after 2000 elections?	2	54	80
	Averages						**3.3**	**57.9**	**69.2**

Note on columns (3) to (6): vote shares of individual parties that ***gained*** power shown in bold

Sources: CRISP (2001 and 2007), http://www.ibzdgip.fgov.be/result/nl/main.html, http://www.bisa.irisnet.be/themas/verkiezingen#.UqXcgDsjJsQ

[229] Municipal elections were always held in October of the election year in question.

Using the terminology of Banzhaf (1968), a large party is hard to exclude from a coalition, because the larger the party the higher its coalition formation power, i.e. the higher the number of coalitions for which its vote share is needed[230]. In the wording of Laver and Shepsle (1996 p. 186): "(…) large parties figure more conspicuously in the decisive structure of the government formation game. They participate more frequently in winning coalitions -an "opportunities" advantage-, and if other parties try to assemble a winning coalition that excludes a large party, negotiations are more prolonged and difficult -a "transaction cost" advantage". The transaction cost advantage is empirically investigated by Martin and Vanberg (2003 p. 330), who find a significantly positive effect of an increase in the number of coalition parties on coalition bargaining duration. Browne and Franklin (1973 p. 462) add as another disadvantage of an anti-coalition that the more coalition parties there are, the larger the extent becomes to which the dominant coalition party will have to cede control over public policy.

We only consider as exogenous the power statuses of the *major* excluded party and the *major* included party in our IV regression analysis based on anti-coalitions. We therefore assume that a "grand coalition" of these 2 largest parties would not have been feasible in our anti-coalition cases, i.e. that each of these 2 largest parties wanted to avoid the other large party as a coalition partner. Also, the *major* excluded party and the *major* included party are both the most *vital* parties to an anti-coalition: an anti-coalition is an anti-coalition *precisely because* a large party has been excluded *and* another large party has delivered the vote share that is indispensable (though not sufficient) for a majority without the excluded large party. A final reason to neglect the junior partners of an anti-coalition is that we assume that these smaller parties would have stood an equally good chance of being included into a coalition by the *excluded* dominant party, in case a "reverse anti-coalition" would have been formed[231].

The 8 anti-coalitions described above are the *only* coalitions over our sample period that we are able to consider as anti-coalitions on the basis of our above criteria. All 4 major political parties at the Brussels municipality level experienced anti-coalitions, either as a "perpetrator" or as a "victim". Remarkably, MR was involved in all of these. Also, being excluded from power resp. being the largest party in power due to an anti-coalition appears a valid and relevant instrument in our IV regressions below including a LDV (table 5), based on the result of the Hansen J test and based on the p values and the F-test of our first stage regressions. This sheer fact should suffice as a motivation for our IV strategy.

Moreover, per individual party the number of anti-coalitions involved in are few -except for MR-, and the number of anti-coalitions "perpetrated" resp. "suffered" of course even fewer. The apparently exceptional nature of an anti-coalition seems to suggest that they are hard to create. However, the limited amount of information these few anti-coalitions contain may put a limit on the precision of our estimates below. Also, if we drop either one of the 3 anti-coalitions PS was involved in from our regression analysis below, the significance of the PS coefficient in our preferred regression specifications behind columns (1) and (3) in table 12 below considerably diminishes[232].

---

[230] Or as Browne and Franklin (1973 p. 461) and Folke (2013 p. 4) state: a large party is more likely to be pivotal, and hence more likely to be part of a governing coalition.

[231] If we however assume that coming to power as part of an anti-coalition is not only exogenous for its largest party *but also for its other members*, the partisan alignment coefficients with respect to MR, PS and CDH in the IV regressions of table 12 below are insignificant.

[232] However, the significantly positive Ecolo alignment effect that we find below is robust against *only* instrumenting Ecolo municipal power participation (while not instrumenting municipal power participation of the other 3 major parties). These Ecolo effects are found by means of an IV for Ecolo municipal coalition

6.2. IV for Ecolo alignment: alignment after 1st ever municipal coalition participation

Ecolo was the "perpetrator" of an anti-coalition or its "victim" in only 1 instance over our sample period, which seems too few to construct an IV. However, we can arguably not afford to leave Ecolo out of our analysis of partisan alignment, as Ecolo is one of the 4 major parties at the Brussels municipal level[233]. Partisan alignment with respect to Ecolo may be correlated not only with grants received but also with alignment with respect to one or more of the other main political parties, as municipal power participation of Ecolo can be expected to reduce the probability of municipal power participation of the other 3 major parties. Therefore, we consider it more prudent to develop a different instrument for alignment with respect to Ecolo, which is moreover based on more observations.

Ecolo is the only one of the 4 major parties that gained power in several Brussels municipalities *for the 1st time in its history* over our sample period. After the 1994 elections it joined municipal coalitions for the first time ever within the Brussels Region, as it acquired power in the City of Brussels and in Schaarbeek. After the 2000 municipal elections, it managed to remain in office in the City of Brussels and in Schaarbeek, and moreover entered into 9 more municipal governments (Anderlecht, Elsene, Ganshoren, Jette, Molenbeek, Sint-Agatha-Berchem, Ukkel, Vorst, and Watermaal-Bosvoorde).

We argue that these 11 new coalition participations –as well as the several considerable vote share increases that preceded and facilitated these coalition participations- are exogenous to previous grants received by the municipalities concerned, for 2 reasons:

1) The new Ecolo municipal coalition participations either preceded or more or less coincided with 1st ever Ecolo coalition participations *at the subsidising levels*. Hence they could hardly have been fostered by grants steered to municipalities by subsidising governments *in which Ecolo was in power*. One could argue that the 9 extra coalition participations gained in 2000 could have been fostered by extra grants obtained by these municipalities thanks to Ecolo entering the Federal government coalition for the 1st time in history after the 1999 Federal elections. This was the 1st time in history that Ecolo entered a government that subsidises the Brussels municipalities[234]. But it seems hard to believe that Ecolo already managed during the 1st year of its 1st ever Federal coalition participation to favour certain Brussels municipalities with sufficient extra grants so as to "buy" considerably more votes for Ecolo during the 2000 municipal elections. Firstly, as argued above, the parties in power pre-2000 in those municipalities *instead of Ecolo at the subsidising level* could have succeeded in claiming the credit for (the public goods provided with) such grants. Secondly, as "local government" mainly belongs to the competencies of the Brussels Regional government, we suspect that the possibilities to steer *Federal* grants to particular Brussels municipalities are rather limited[235]. Thirdly, while Jennes and Persyn (2015 p. 185) do find that Belgian federal ministers are able

participation that consists of substantially more observations –i.e. 11 new Ecolo coalition participations- than the anti-coalition IVs considered per individual party. (see below)

[233] Moreover, regression coefficients reported below change considerably when leaving Ecolo out of the analysis.

[234] Only in 2004 Ecolo entered for the 1st time the government of the Brussels Region and only in 2009 it entered for the 1st time the government of the French Community.

[235] Column (8) of table 12 (see below) shows that Ecolo participation in the Brussels Regional government –from 2004 onwards- rather than to the Federal government –from 1999 onwards- explains the Ecolo alignment effect.

to steer extra transfers to (citizens living in) their electoral districts with a time lag of one year only, they also find that the medium and long-term effects of such ministerial efforts are much larger.

However, possible endogeneity of municipal coalition participation with respect to previous grants is the reason that we *do* leave out of our instrument the 3 1st ever coalitions participations of Ecolo as a result of the *2006* municipal elections, as they could well have been endogenous to previous grants steered by subsidising governments in which Ecolo was in power.

2) The steep increases in vote share that facilitated the 9 new Ecolo municipal coalition participations in 2000 (see table 11) can be argued to be a case in point of previous research findings that local elections are decided on the basis of *national or federal* preferences for certain political parties. Such a "tide effect" or "trend effect" is e.g. found for municipalities in the Netherlands by Allers and Merkus (2013). In such cases municipal coalition participation would be exogenous to previous municipal grants received. Table 11 shows that this 2000 Ecolo "tide effect" could be more likely than any "tide effects" for other parties[236]. While Ecolo more or less doubled its share of the Francophone vote between 1995 and 1999 both at the Federal and the Brussels Regional level, Ecolo also more or less doubled its average share of the vote between 1994 and 2000 at the Brussels municipal level. Hence the Ecolo 2000 municipal result seems to have benefited from the Ecolo 1999 country-wide results[237].

Table 11: The tide-effect on the Ecolo and CDH 2000 municipal election results in terms of vote shares (%)

	1991 Federal	1994 municipal	1995 Federal	1995 Regional	1999 Federal	1999 Regional	2000 municipal
MR	25.9	34.2	28.5	42.6	29.0	42.7	35.6
PS	36.5	18.8	32.9	26.1	29.0	19.9	20.6
CDH	20.8	14.6	21.3	11.3	16.8	9.8	11.1
Ecolo	13.8	8.4	11.0	11.0	21.0	22.7	17.7

Sources: http://www.ibzdgip.fgov.be/result/nl/main.html, http://www.bisa.irisnet.be/themas/verkiezingen#.UqXcgDsjJsQ, own calculations.

Therefore we will instrument the total of 19 Ecolo municipal coalition participations over our sample period with the 11 Ecolo municipal coalition participations that were the 1st ever per municipality in its history. Our Ecolo instrument is reminiscent of the instrument used in Knight's (2005 p. 1649-1650) study of the effect of representation on the US Congress Committee on Transportation and Infrastructure on transportation grants received. Knight instruments a US electoral district's representation on this Committee with the presence of *newly elected* members of Congress on that same Committee, as this Committee substantially expanded in terms of number of members between 1991 and 1998, and as districts represented on the Committee in 1991 "may have chosen to transfer

[236] Except possibly for CDH, which may have experienced a decrease in vote share at the 2000 municipal elections because of the loss in vote share at the preceding 1999 Federal and Brussels Regional elections.

[237] In turn, the very favourable Ecolo 1999 Francophone-Belgium-wide election results may have benefited from the Belgium-wide food scare that dominated the final months of the 1999 election campaign. Both the Flemish Green party and Ecolo managed to claim the issue of "safe food" as theirs in the run-up to the 1999 elections.

off of the Committee" in 1998[238]. In other words, Committee representation of existing members may have been endogenous to grants, rather than or together with the other way around.

Our instrument for Ecolo looks somewhat more solid than our anti-coalition instruments for the other 3 main parties, as the Ecolo instrument is based on more observations.

6.3. Construction of the regression equation

As a next step we construct our regression equation. Our IV regression equation estimated in table 12 below can be summarised as follows:

$$grantspc_{it} =$$

$$\alpha_i + \alpha_t + \alpha_1\, subsgovt_{it}$$

$$+ \alpha_2 \begin{pmatrix} mungovt_{it} + mungovt_{it} * subsgovt_{it} \\ = anticoalition_{it} * mungovt_{it} + anticoalition_{it} * mungovt_{it} * subsgovt_{it} + anticoalition_{it} \end{pmatrix}$$

$$+ \alpha_3 \begin{pmatrix} Ecolo\ mungovt_{it} + Ecolo\ mungovt_{it} * Ecolo\ subsgovt_{it} \\ = new\ Ecolo\ mungovt_{it} + new\ Ecolo\ mungovt_{it} * Ecolo\ subsgovt_{it} \end{pmatrix}$$

$$+ \alpha_4 X_{it} + \varepsilon_{it}$$

With:

α_i a municipality fixed effect,

α_t a year fixed effect

With $subsgovt_{it}$ the count variable measuring being in power in subsidising governments, taking integer values between 0 and 3, resp. for MR, CDH and Ecolo[239]

[238] This may have been the case because the pork barrel those districts received in 1991 may have caused them to experience diminished marginal utility from transportation projects in 1998.

[239] We do not develop an IV strategy to take into account possible endogeneity of the "subsgovt" variables. We hence implicitly assume that the "subsgovt" variables are exogenous with respect to past grants received at the municipal level, as opposed to the "mungovt" variables.
This assumption seems reasonable for the Federal government and the French Community government. Indeed, parties may gain power at the Federal and French Community government level only to a limited extent because a number of Brussels municipalities received more grants, due to which subsidising parties would increase their vote share during subsequent Federal and French Community elections. E.g. in the 1999 Federal election only 11 of the 59 Francophone seats in the Federal House of Representatives were provided by the electoral district including the Brussels Region. This reasoning is in line with the reasoning e.g. Albouy (2013 p. 127) develops for the US.
But this reasoning of course does not hold for the Brussels Region elections. One can easily imagine that grants steered to Brussels municipalities by the Brussels Regional government could considerably affect the outcome of Brussels Regional elections. This is a weakness of our empirical strategy because the partisan alignment effects we find below seem to be driven by the Brussels Regional level.

With:

$$\begin{pmatrix} mungovt_{it} + mungovt_{it} * subsgovt_{it} \\ = anticoalition_{it} * mungovt_{it} + anticoalition_{it} * mungovt_{it} * subsgovt_{it} + anticoalition_{it} \end{pmatrix}$$

being a sum including as terms:

1) the "party in municipal power" dummy instrumented by the "party in municipal power *after the formation of an anti-coalition*" interaction variable, resp. for MR, CDH and PS
2) the interaction variable of interest, i.e. the "aligned party in municipal power" interaction variable instrumented by the "aligned party in municipal power *after the formation of an anti-coalition*" triple interaction variable, resp. for MR and CDH; for PS the variable of interest is just the PS "party in municipal power" dummy multiplied by 3

With:

$$\begin{pmatrix} Ecolo\ mungovt_{it} + Ecolo\ mungovt_{it} * Ecolo\ subsgovt_{it} \\ = new\ Ecolo\ mungovt_{it} + new\ Ecolo\ mungovt_{it} * Ecolo\ subsgovt_{it} \end{pmatrix}$$

being a sum including as terms:

1) the "Ecolo in municipal power" dummy instrumented by the "Ecolo *new* in municipal power" interaction variable
2) the "Ecolo aligned party in municipal power" interaction variable instrumented by the "Ecolo aligned party *new* in municipal power" interaction variable

And with X_{it} a set of financial, demographic and political control variables

Our empirical approach contrasts with the bulk of both the "1st generation" and the "2nd generation" literature on partisan alignment –discussed above- in 3 ways:

1) We include alignment variables for *multiple individual parties into one single regression*, rather than regressing grants on a *general* alignment variable, as is common in the partisan alignment literature. In this respect, our research set-up is as far as we know only similar to the research of Rodden and Wilkinson (2004) and Khemani (2007) on India, Rodden (2005) on Germany, Migueis (2013 p. 20) on Portugal, and of Fiva and Halse (2014) on Norway, as these studies also investigate individual party effects. Investigating individual party effects seems interesting, as Folke (2013 p. 2) writes: "Despite the prominence of the assumption that parties matter there is no systematic evidence on how the representation of individual parties affects policy outcomes in proportional representation systems." E.g. Bracco e.a. (2012), Brollo and Nannicini (2012), and Curto-Grau e.a. (2012) not only differ from our study in that they investigate the effect of alignment of a lower level government *with respect to a party block rather than with individual political parties*, but also in that they do not distinguish between the alignment effects according to a leftwing block and the alignment effects according to a rightwing block.

2) We take care to explicitly construct our alignment variables as *interaction variables*[240], again in contrast to e.g. Bracco e.a. (2012), Brollo and Nannicini (2012), and Curto-Grau e.a. (2012). These latter studies hence do not construct their alignment variable as the interaction of a dummy measuring being in power of a particular –i.e. either leftwing or rightwing- block at the lower government level with another single dummy measuring being in power of a particular –i.e. either leftwing or rightwing- block at the higher government level. We believe that it is an important contribution of our analysis to the literature that we construct alignment as an interaction, hence additionally including the *factors* that compose the interaction as *separate regressors*. When not including these separate regressors, a significant coefficient of our interaction variable may even represent a spurious correlation. (see e.g. Brambor e.a. 2005 p. 66-71) Alternatively, without including the separate regressors an insignificant coefficient of the interacted variable may as well erroneously point at the *absence* of any correlation[241]. One could e.g. argue that our dataset of only 312 observations would rather warrant a more parsimonious specification, i.e. a specification that does not include the factors of which the interaction is composed as separate regressors. Remarkably, such a specification makes all coefficients of interest in our regression analysis below turn insignificant (see below). This is in line with the finding of Brambor e.a. (2005 p. 79) that omitting constitutive factors of the interaction variable as separate regressors may either overestimate or underestimate the coefficient of the interaction variable.
   What is more, in what follows it will appear that we will be able to give the coefficient on one of the constitutive factors of our interaction variable of interest, i.e. the respective "mungovt" variables, a rather important interpretation from a partisan alignment perspective. This coefficient measures the effect of a municipality "including a particular party into its coalition" on grants received whenever this party is *not* part of one of the coalitions at the subsidising levels, i.e. it measures a kind of "reverse alignment effect" (or "punishment effect for not being aligned").
3) Our resp. "subsgovt" variables are *count variables* rather than simple dummy variables, as they count the number of subsidising governments in which a party in question simultaneously participated-, hence taking a value between 0 and 3[242]. Therefore, our interaction variable in question measures per individual party the *degree* of partisan alignment between a municipality and the subsidising government levels.

[240] Except for PS, for which this is impossible due to the *constant* coalition participation of PS at the 3 subsidising levels over our sample period.
[241] Admittedly, a spurious correlation would be worse than the erroneous suggestion of absence of any correlation.
[242] As said above, because we do not know the resp. shares of the 3 subsidising levels in total discretionary grants, we simply increase our count variable by 1 for each extra subsidising level at which a particular party is in power. The aggregate nature of our grants data –not distinguishing between the 3 governments that subsidise the Brussels municipalities- makes the use of our "subsgovt" count variables less innovative compared to the existing literature than that it otherwise could have been.

7. Regression results

Regression results are shown in table 12 below.

Column (1): FE regression, with the alignment variables of interest as simple dummies

As a start, column (1) of table 12 presents the simple OLS Fixed Effects (FE) regression result of grants on alignment, with the alignment interaction variables of interest modelled as simple dummies, which take the value of 1 in case of a municipality characterised by the party in question being at the same time in power at the municipal level and at one or more subsidising levels. As said above, all coefficients of interest are insignificant in this specification.

We also include municipality and year dummies, the estimates of which we do not report, for reasons of space. We additionally include a number of rather standard financial and economic-demographic controls at the municipal level, similarly to the controls used by e.g. Bracco e.a. (2012 p. 18). In all regressions reported in table 12, the 3 economic-demographic variables included are the number of unemployed, young, and elderly expressed per 100 inhabitants, and the 3 financial variables are private income, municipal debt and municipal tax revenues expressed in 2011 euros per capita[243].

Remarkably, hardly any of the added financial and demographic controls appears significant. This could be due to the discretionary nature of the grants considered, i.e. the special purposes for which they are meant –e.g. urban renewal- could make their attribution rather unrelated to our control variables. Alternatively, it could mean that political and tactical considerations dominate needs considerations with respect to the allocation of discretionary grants, similar to the conclusion drawn with respect to discretionary grants to Italian municipalities by Bracco and Brugnoli (2014 p. 12). The low significance of our control variables, also compared to our variables of interest, persists in our IV analysis (see below).

We also control for the resp. municipal vote shares[244]. Effectively, partisan alignment could be endogenous with respect to vote share: a particular party could have gained power at the municipal level thanks to the popularity of its local candidates, who provided their party with a high vote share, possibly because they managed to attract many grants, possibly for other reasons. Additionally, we include interactions of municipal vote shares with the dummy measuring at how many subsidising levels a party is in power. These are meant to check if extra grants are allocated by incumbents at the subsidising levels to municipalities where these incumbents' parties obtain high vote shares during municipal elections (the standard partisan hypothesis) rather than to politically aligned municipalities. Hardly any of the political control variables appears significant, in particular as we refine our regression specification moving from left to right in table 12.

[243] Unemployment data have been obtained from the Federal Ministry of Employment; demographic and income data have been obtained from Statistics Belgium (www.statbel.fgov.be); debt and tax revenue data have been obtained from Belfius.

[244] In what follows, data on vote shares have been obtained from http://www.ibzdgip.fgov.be/result/nl/main.html and http://www.bisa.irisnet.be/themas/verkiezingen#.UqXcgDsjJsQ.

Table 12: Regression of discretionary grants (in euro per capita) on alignment[245]

	(1) Grantspc **FE Alignment as a simple dummy**	(2) Grantspc **FE Alignment as a simple count variable**	(3) Grantspc **FE Alignment as an interaction**	(4) Grantspc **IV regression (anti-coalitions and 1st Ecolo coalitions)**	(5) D.grantspc **IV again, but now including LDV as a regressor**	(6) D.grantspc **Being in power weighted for municipal vote share**	(7) D.grantspc **Being in power weighted for municipal coalition formation power index**	(8) D.grantspc **Distinguishing Ecolo alignment per subsidising government**
(D.)L.grantspc					**0.333**	**0.533****	**0.733****	**0.364**
					(0.347)	**(0.257)**	**(0.340)**	**(0.268)**
(D.)MRmungovt			**26.59**	**-83.18**	**99.40**	**6.094**	**1.689**	**114.9**
			(21.29)	**(84.29)**	**(201.3)**	**(4.535)**	**(1.877)**	**(224.2)**
(D.)MRsubsgovt			0	-32.99	113.6*	3.146	32.24	49.19
			(.)	(40.00)	(63.01)	(45.21)	(29.97)	(170.6)
(D.)MRmungovt *subsgovt	**-6.551**	**0.161**	**-26.58****	**36.04**	**-150.2**	**-2.645**	**-0.326**	**-117.7**
	(6.347)	**(14.78)**	**(12.49)**	**(46.07)**	**(130.6)**	**(2.495)**	**(1.005)**	**(130.1)**
(D.)CDHmungovt			-33.14	16.02	-303.2	-0.323	1.579	**-214.9**
			(22.01)	(83.07)	(276.0)	(4.782)	(2.002)	**(227.3)**
(D.)CDHsubsgovt			-58.27**	-75.78**	-49.84	7.435	3.158	-118.8**
			(26.05)	(38.10)	(56.22)	(17.91)	(22.77)	(57.50)
(D.)CDHmungovt *subsgovt	**2.360**	**5.676**	**12.73***	**2.858**	**123.4**	**2.942**	**0.476**	**112.2**
	(5.418)	**(11.84)**	**(6.399)**	**(30.51)**	**(106.7)**	**(2.563)**	**(1.168)**	**(120.5)**
(D.)ECOLOmungovt			**-32.97**	**-73.29****	**-202.8*****	**-10.75*****	**-0.854**	**-187.8*****
			(26.36)	**(29.45)**	**(64.24)**	**(3.800)**	**(0.889)**	**(55.41)**
(D.)ECOLOsubsgovt			-17.30	-28.38	-47.82	13.88	-18.26	
			(28.71)	(32.76)	(64.22)	(58.29)	(72.38)	
D.ECOLOfedgovt								-87.16
								(118.8)
D.ECOLOreggovt								-113.2
								(222.8)
D.ECOLOcommgovt								-134.0
								(163.3)
(D.)ECOLOmungovt *subsgovt	**4.669**	**8.959**	**25.33**	**56.65*****	**95.80****	**8.334*****	**0.904***	
	(11.30)	**(15.89)**	**(19.89)**	**(21.61)**	**(44.31)**	**(3.141)**	**(0.542)**	
D.ECOLOmungovt *fedgovt								**54.14**
								(43.82)
D.ECOLOmungovt *reggovt								**141.8***
								(76.07)
D.ECOLOmungovt *commgovt								**111.5**
								(608.9)
(D.)PSmungovt*3	**-1.387**	**-1.411**	**-2.481**	**13.05****	**19.82**	**1.458***	**0.676****	**21.19***
	(4.342)	**(4.758)**	**(5.211)**	**(6.398)**	**(12.48)**	**(0.783)**	**(0.306)**	**(10.62)**

[245] For reasons of space, the interaction variables of table 12 only mention once –and not twice- the political party to which they are related. E.g. we note "MRmungovt*MRsubsgovt" as "MRmungovt*subsgovt".

(continuation of table 12 from previous page)

(D.)incomepc	0.0149	0.0149	0.0125	0.0207	0.0136	0.0272	0.00540	-0.0123
	(0.0197)	(0.0199)	(0.0207)	(0.0152)	(0.0282)	(0.0284)	(0.0291)	(0.0307)
(D.)unemployedpc	16.89	15.15	14.16	15.52	-71.83**	-70.73**	-61.99*	-63.78*
	(22.84)	(22.86)	(21.05)	(19.02)	(33.67)	(33.11)	(35.98)	(34.81)
(D.)under18pc	13.50	12.92	12.86	30.88**	44.27	32.89	8.621	-12.56
	(8.063)	(9.112)	(8.684)	(13.05)	(34.88)	(29.96)	(26.12)	(51.26)
(D.)over65pc	-0.255	-0.354	-0.971	4.478	-14.99	-40.95	-51.38	-59.46
	(7.516)	(7.713)	(7.371)	(7.211)	(42.81)	(33.07)	(35.93)	(40.65)
(D.)debtpc	0.00852	0.00822	0.0111	0.0235	0.0328	0.0270	0.0640	0.0380
	(0.0134)	(0.0135)	(0.0143)	(0.0188)	(0.0602)	(0.0571)	(0.0576)	(0.0604)
(D.)taxrevenuepc	0.0608	0.0665	0.0496	0.104	-0.120	-0.117	-0.157	-0.0833
	(0.0615)	(0.0653)	(0.0633)	(0.0661)	(0.130)	(0.0912)	(0.0994)	(0.106)
(D.)MRmunvote	1.932**	1.993*	1.104	3.740**	-2.436	-2.561	0.632	2.878
	(0.872)	(0.957)	(1.148)	(1.890)	(6.020)	(4.738)	(3.695)	(2.885)
(D.)CDHmunvote	-1.861	-1.984*	-2.176*	-1.020	-4.248	2.418	2.509	2.437
	(1.086)	(1.115)	(1.143)	(1.468)	(7.245)	(2.582)	(2.894)	(3.347)
(D.)ECOLOmunvote	-4.835	-4.795	-5.761	-5.922	-15.30	4.977	-0.139	-12.60
	(4.502)	(4.376)	(4.292)	(4.976)	(13.36)	(6.880)	(9.084)	(12.36)
(D.)PSmunvote	-1.388	-1.092	-2.044	-3.109	-9.479	-3.733	1.696	-3.684
	(0.968)	(1.076)	(1.226)	(2.054)	(9.052)	(3.553)	(3.576)	(6.297)
(D.)MRmunvote	-0.556**	-0.599**	-0.391	-0.964*	0.185	2.411	0.137	-0.446
*subsgovt	(0.245)	(0.262)	(0.273)	(0.509)	(1.005)	(2.820)	(1.822)	(0.600)
(D.)CDHmunvote	0.480	0.510	0.396	0.614	-0.617	-2.383	-1.056	-0.586
*subsgovt	(0.375)	(0.379)	(0.393)	(0.469)	(0.866)	(1.840)	(2.107)	(0.599)
(D.)ECOLOmunvote	3.676	3.672*	3.795	4.293**	4.443	-0.377	4.756	9.112
*subsgovt	(2.126)	(1.983)	(2.221)	(2.005)	(3.685)	(3.715)	(4.566)	(6.168)
N	312	312	312	312	255	255	255	255
Hansen J statistic (p value)				0.05	0.93	0.71	0.24	0.64
F-test of joint significance of IVs in 1st stage (p values)				0.00	0.00	0.00	0.00	0.00
Number of IVs				9	11	11	11	11

Robust standard errors clustered at the municipality level in parentheses. Clustering at the municipality-legislature level generates highly similar regression results.

Year dummies included; municipality dummies included only in columns (1) till (4).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column (2): FE regression, with the alignment variables of interest as a simple count variable

As a next step, column (2) of table 12 presents the simple OLS FE regression result of grants on alignment, with the alignment interaction variables of interest now modelled as count variables, which take the value of 1, resp. 2, resp. 3 in case of a municipality characterised by the party in question being at the same time in power at the municipal level and at 1, resp. 2, resp. 3 subsidising levels. Again, all coefficients of interest are insignificant in this specification.

Column (3): FE regression, including the constitutive factors of the alignment interaction variable of interest as separate regressors

Column (3) of table 12 then presents the simple OLS FE regression result of grants on alignment, using the full specification as presented in section 5.3, i.e. including the constitutive factors of the alignment interaction variable of interest as separate regressors. The impact of this inclusion is considerable, as several of our alignment coefficients now become significant or at least increase in size.

The coefficient of the CDH variable of interest now becomes significantly positive with a p value of 0.06, and the Ecolo variable of interest now increases but remains insignificant with a p value of 0.22. The PS coefficient of course remains very small and very insignificant. Surprisingly, the coefficient of the MR variable of interest becomes significantly negative. It implies an average grants *de*crease of 26.6 real euros per capita and per year for every municipality in which MR is part of the coalition, whenever MR joins a coalition at a subsidising government level[246]. This negative association is hard to interpret. One possibility is that a *decrease* in grants to municipalities in which MR is not part of the coalition, leads to MR becoming part of the coalition. However, the negative sign persists also in most of our IV analysis below, which attempts to isolate the *causal effect of partisan alignment on grants.*

A 2nd possible explanation for the negative sign of the MR coefficient of interest is the one given by Baskaran and Hessami (2014) for their finding that rightwing state governments favour *unaligned* municipalities at the expense of aligned municipalities in the German state of Hesse. As said above, their explanation is that in their panel only few municipalities are aligned with rightwing state governments, due to which these state governments would have been compelled to "buy off" the support of unaligned municipalities with more discretionary grants (possibly reserving other kind of benefits than discretionary grants for aligned municipalities). This explanation may apply to the situation of the Brussels municipalities too. Table 5 above shows that MR is the only centre-right party of the 4 major Brussels parties, and hence may have policy preferences that diverge from the other 3 major Brussels parties[247]. Also, while table 4 shows that MR is the party that participated most in Brussels municipal coalitions (45 times), the 3 major (centre-)left parties taken together unsurprisingly participated even more often in Brussels municipal coalitions (80 times). Moreover, MR is strongest in the richest Brussels municipalities, which may not *need* discretionary grants as much as poorer municipalities[248], which in turn may have

[246] Or for every subsidising government level at which MR is part of the coalition, once MR joins the coalition in a particular municipality.

[247] Although we are unable to detect an ideological pattern in MR's municipal as well as subsidising government coalition preferences (see table 9).

[248] This point is related to the point made by Engstrom and Vanberg (2010 p. 982) that in the US geographically targeted earmarks may be associated more with the Democratic party than with the Republican party because of the different preferences of the resp. politicians: relatively leftwing politicians may *demand* more earmarks than relatively rightwing politicians.

enabled MR to favour non-aligned municipalities with more discretionary grants in return for one or more other policy favours.

Thirdly, the negative sign of the MR coefficient of interest may be related to the fact that MR has been a –sometimes uneasy- alliance of 2 parties, rather than a single party, which may have weakened its alignment effect. Indeed, MR is the alliance that existed from 1992 until 2011 between the 2 Francophone Belgian parties PRL and FDF, including at the municipal level (though the name MR only dates from 2002). In most occasions over our sample period where both PRL and FDF had campaigned on separate lists at the *municipal* level and had both obtained seats, PRL and FDF were either both part of a municipal government or were both in opposition. However, in particular during the first years of our sample period some municipal legislatures were characterised by PRL being part of the coalition and FDF being part of the opposition, or vice versa[249]. However, this 3rd argument could be an explanation for an *insignificant* coefficient rather than for a significantly *negative* coefficient.

A final explanation of the negative sign of the MR coefficient of interest is that it is sometimes said of Belgian center-right parties such as PRL and MR that they lack the organisational strength of the centre and centre-left parties. The degree of political professionalism seems to be higher in rather leftwing parties. E.g. already within PRL taken separately, there happened to be rather more internal conflicts and breakaway movements in several municipalities over our sample period than within the other 3 parties. However, again this could be an explanation for an insignificant coefficient rather than for a significantly negative coefficient.

Hence, on the basis of our FE regression analysis, evidence for the partisan alignment hypothesis seems weak, with even a –hard to explain- significantly negative association between MR alignment and grants, and with –rather surprisingly- the 2 smaller parties CDH and Ecolo showing the most positive association between alignment and grants.

Column (4): IV regression, with alignment after the formation of an anti-coalition and after 1st ever Ecolo municipal coalition participation as IVs

As a next step, we use alignment status after the formation of anti-coalition, and –for Ecolo- alignment status after 1st ever coalition participations as an IV in a regression of grants on overall alignment status. The resulting change in our coefficients of interest compared to the FE regression outcomes of column (3) is considerable:

- the coefficient for the variable of interest for MR turns positive and insignificant
- the coefficient of the CDH variable of interest remains positive, but turns insignificant
- the coefficients of the Ecolo and PS variables of interest turn significant at the 0.01, resp. 0.05 level with the expected sign, and their magnitudes increase considerably; However, the size of the Ecolo coefficient is far larger than the size of the PS coefficient, which is surprising given the large positive difference between PS and Ecolo municipal vote share as well as between the number of PS and Ecolo coalition participations at all municipality-subsidising levels over our sample period (also see tables 3 and 4 above)

[249] In these rare cases, we considered MR as being part of the coalition c.q. opposition if its largest component in terms of vote share –either PRL or FDF- was part of the coalition c.q. opposition.

All coefficients of interest in column (4) are larger than the FE coefficients in column (3), except the CDH coefficient. This is surprising, as we would expect OLS estimates to be upwardly biased, mainly because of simultaneity, i.e. reverse causality between grants and being in power[250]. However, the reverse causality bias of our above FE analysis may be rather small given the even larger variation in our independent variables of interest compared to the variation in our dependent variable of interest, as can be concluded from a comparison of their standard deviations (not shown).

Moreover, the higher coefficients resulting from our IV regression could be explained by the *nature* of our IVs. Although alignment after an anti-coalition or because of a 1st ever municipal coalition participation can be argued to be exogenous to previous grants received, subsidising governments may ex post particularly favour "anti-coalition municipalities" and "1st ever" municipalities with extra grants. Otherwise stated, *exogeneity* of our instruments does not guarantee full *comparability* of the observations on which our instruments are based. The coefficients of interest in column (4) may hence be an overestimation.

The interpretation of the Ecolo (PS) coefficient of interest is now that whenever in power at one of the subsidising government levels, Ecolo (PS) steers extra grants to any Brussels municipality in which it is also in power. An alternative interpretation could however be that whenever having gained power in a Brussels municipality, Ecolo (PS) manages to receive extra grants from those subsidising governments at which level it is also in power. This estimation result hence provides support for the partisan alignment hypothesis rather than for the standard partisan hypothesis: as the Ecolo (PS) vote share variable has an insignificant or only small coefficient, it is the fact that the *Ecolo (PS) party* is in power at the municipal level that determines if the Ecolo (PS) party in power at the subsidising levels steers extra grants, rather than the presence of many *Ecolo (PS) voters*. Still otherwise stated: the presence of many Ecolo (PS) voters does not seem to be sufficiently reassuring for Ecolo (PS) to steer extra grants, because if the municipality was non-aligned in spite of many sympathetic voters, Ecolo (PS) would e.g. fear that either the extra grants may not reach those voters or that the party (parties) in power at the municipal level may claim the credit for the extra grants rather than Ecolo (PS) at the subsidising level (who caused the extra grants to be provided in the first place). Because our dependent variable –crucially- concerns *intergovernmental* grants, Ecolo (PS) -when in power at the subsidising level(s)- is only reassured of "being awarded the credit" if Ecolo (PS) is also in power in the municipality in question.

To be able to compare with alignment effects found in the existing partisan alignment literature, we calculate our average effect of partisan alignment as follows: for Ecolo we take the sum of its alignment coefficient (as shown in column (4)) and of that same alignment coefficient multiplied by 2, after weighting each of these 2 terms with the relative frequency of Ecolo being in power at 1 subsidising level, resp. 2 subsidising levels, as over our sample period Ecolo was never in power at all 3 subsidising levels at the same time. This sum is: {56.7*(170/312)}+{(56.7*2)*(38/312)}. This sum amounts to 44.8 euros

[250] In their regression of discretionary grants to Canadian electoral districts on political-economy variables, also Milligan and Smart (2005 p. 15-16) obtain higher coefficients for their political variables of interest when instrumenting them. They are equally puzzled when trying to explain this result. Similarly, as said above, when refining the Solé-Ollé and Sorribas-Navarro (2008) paper by replacing an OLS design with a (fuzzy) RDD –which implies performing an IV regression-, Curto-Grau e.a. (2012) find an even stronger positive effect of party-political alignment on grants received by municipal governments than in their 2008 study. Also Knight's (2005 p. 1650) effect of US Congress Committee representation on pork barrel spending increases after instrumenting Committee representation, although its significance decreases.

(with a standard deviation of 17.4 euros and a p value of 0.01), or 20.7% of the annual per capita grant amount of 216.7 real euros of an average municipality. For PS the average alignment effect is obviously 13.1 euros, or 6.0% of the annual average per capita grant amount of 216.7 real euros.

However, we should draw a different conclusion from the PS coefficient of interest obtained in column (4), because we are not able to distinguish any subsidising-level-induced effect of a change in PS alignment. Indeed, over our sample period, PS has *always* been in power at every single of the 3 subsidising government levels. Firstly, for PS the conclusion should perhaps simply be that municipalities that include PS in their coalition obtain more grants, *whether PS is also in power at the subsidising level(s) or not*. This could be because PS could simply (be able to) attract staff at the municipal level that is particularly *able* to obtain grants from the various subsidising levels, or particularly *motivated* to do so because of the preferences of its voters. In other words: the PS coefficient of interest may not be a correct measure of the effect of partisan *alignment*, but rather a measure of a different kind of politicisation of grant allocation[251]. Secondly, we should perhaps even refrain from deriving *any interpretation at all* from the PS coefficient, as Brambor e.a. (2005 p. 79) show that omitting the constitutive factors of an interaction variable as separate regressors –as we are forced to do in the case of PS- may *either overestimate or underestimate* the coefficient of the interaction variable.

Importantly, also the *"mungovt" component* of our MR, CDH, and Ecolo "mungovt" – "subsgovt" interaction variables included as a separate regressor can be interpreted. The coefficient of the "mungovt" variables for those 3 parties shows the effect of entering into a municipal coalition on grants *in cases where there is no alignment at all* with the subsidising levels. The expectation is that the "mungovt" coefficients be negative, as a party in power at a subsidising level seems to have an incentive to direct grants *away* from municipalities in which it is not in power. Hence the "mungovt" coefficients measure some kind of *reverse alignment* effect. The expectation of a negative coefficient is confirmed by most coefficients concerned in table 12 except for MR.

However, the "ECOLOmungovt" coefficients in the resp. columns of table 12 are the only "mungovt" coefficients that are *significantly* negative in almost every column from column (4) onwards. The size of the "ECOLOmungovt" coefficients is even larger than the size of the Ecolo alignment coefficients[252]. It implies that when a municipality takes Ecolo into its coalition while Ecolo is *not* in power at any of the subsidising levels, it receives significantly *less* grants. This effect can be interpreted as some sort of "punishment" of municipalities in which Ecolo is in power by subsidising levels that are "not friendly with Ecolo". One explanation of such "punishment" could be that of our 4 parties Ecolo is the youngest political party and is perceived the "non-traditional" party of the 4 parties considered. The 3 other political parties may view Ecolo as an "intruder", that threatens their "share of the market for votes", and against which a decrease in discretionary grants may be meant as a "barrier to entry". Alternatively, the significantly negative "ECOLOmungovt" coefficient can be interpreted as lower Ecolo skills or

[251] Alternatively, one could argue that it would precisely be the constant hold on power of PS in any subsidising government since 1988 that would cause the significantly positive PS coefficient of interest in column (4). This coefficient could mean that PS has gained strong knowledge with respect to the tactical –i.e. partisan- allocation of discretionary grants to municipalities.

[252] The punishment of municipalities where Ecolo is in power may have contributed to Ecolo's loss of vote share between the 2000 and the 2006 municipal elections from on average 17.7 to 13.3%.

motivation to attract grants when in municipal power, possibly due to Ecolo's inexperience[253]. Both explanations may be accentuated by the fact that we instrument Ecolo municipal coalition participations with *first ever* Ecolo municipal coalition participations *only*.

Remarkably, there does not seem to exist any political economy or political science literature on the effect of being (in)experienced/new as a political party in power on grants received. With respect to the significantly *positive* alignment coefficient for Ecolo, there *does* exist a considerable literature on the –*positive*- effect of being inexperienced/new *as an individual politician* in power on grants received. An example is Milligan and Smart (2005 p. 11), who find that electoral districts of Canadian ministers receive *more* grants the *less* parliamentary experience these ministers have, as Canadian political parties particularly wish to electorally support their inexperienced ministers. Similarly, Engstrom and Vanberg (2010) find that districts of *freshmen* members of the US Congress get allotted *more* earmarks. With respect to the significantly *negative* "mungovt" coefficient for Ecolo, i.a. Ryan (2008) finds that districts receive *less* federal funding if they are represented by a US member of Congress who has *shorter* tenure.

Additionally, the "mungovt" variables enable us to calculate –rather than the average alignment effect *in isolation*- the *average effect of "a municipality taking a particular party into its coalition" in general on grants received*. The calculation of this effect is shown in table 13, and follows the method suggested by Brambor e.a. (2005 p. 72-74). In column (0) we simply repeat the average effect of *alignment as such* as calculated above. In column (1) we simply repeat the regression results for the "ECOLOmungovt" coefficient in column (2) of table 12. In columns (2) and (3) of table 13 we calculate the average effect of taking Ecolo in one's municipal coalition if -in contrast to column (1)- Ecolo is also present in 1 resp. 2 subsidising government(s). This latter effect is the sum of the "ECOLOmungovt" and "ECOLOsubsgovt* mungovt" coefficients of column (4) in table 12 –with the "ECOLOsubsgovt*mungovt" coefficients multiplied by 2 in column (3) of table 13. Finally, we obtain the average effect of a municipality "taking Ecolo into its coalition" on grants received in column (4), by summing up the coefficients of columns (1) until (3) after weighting each term by its sample share (bottom row in table 13). The conclusion is that this average effect is negative but insignificant, so that *"on a net basis"* municipalities in which Ecolo is in power do not receive more or less grants. This is because the subsidising level(s) at which Ecolo is in power manage(s) to compensate for the loss of grants Ecolo municipalities experience whenever Ecolo is not in power at any of the subsidising levels, and/or vice versa.

Table 13: Average effect of a municipality taking Ecolo into its coalition on discretionary grants received (in euro per capita) over our sample period

	(0) average alignment effect in isolation	(1) total effect if "ECOLOsubsgovt" = 0	(2) total effect if "ECOLOsubsgovt" = 1	(3) total effect if "ECOLOsubsgovt" = 2	(4) average total effect
coefficient	**44.8**	**-73.29**	-14.1	42.7	**-26.2**
standard error	**17.4**	**29.45**	29.6	39	**29.4**
p value	**0.01**	**0.04**	0.63	0.27	**0.37**
sample share	312/312	104/312	170/312	38/312	312/312

Source: own calculations

[253] Another possible explanation could be different ethical standards of Ecolo politicians, as Ecolo has also been known for striving towards "a more clean political culture".

At first sight, also the coefficients of the *separate "subsgovt" component* of our MR, CDH, and Ecolo "mungovt" – "subsgovt" interaction variables included as a separate regressor seem fit for interpretation. These coefficients seem to show the effect of a particular party entering a coalition at some subsidising government level on grants to municipalities in which this party is *not* part of the coalition. I.e. at first sight these coefficients seem to be an indicator of parties' ability or willingness to "punish" non-aligned municipalities with less grants. However, our 3 "subsgovt" count variables are collinear with one or more of our year dummies included into our regression specification[254]. This is because –evidently- all of our "subgovt" count variables only vary over time –i.e. they do not vary at all between municipalities-, similarly to our time dummies. Hence, the 3 "subsgovt" count variables may pick up *other* influences on grants apart from the effect of a change in coalition participation of a particular party at the subsidising levels. However, the fact that we cannot interpret the coefficients of our "subsgovt" variables should *not* prevent us from interpreting the coefficients of our *actual* variables of interest, i.e. of the alignment interaction variables, as these interaction variables *do* consist of a variable that varies between municipalities (i.e. our "mungovt" variable).

All our instruments are relevant. This is noticeable from the 1st stage of the IV regression –not shown in table 12- in which the p values of their (positive) coefficients are 0.000, as well as the p values of the F-tests of joint significance of the IVs -shown at the bottom of table 12. However, the Hansen J test does not allow us to reject the hypothesis that our instruments are endogenous, with a p value of 0.05[255]. Because of the latter result, we will run an alternative IV regression in column (5) of table 12, including a lagged dependent variable (LDV). This will be our preferred specification, as the Hansen J test it produces does allow us to reject the hypothesis that our instruments are endogenous.

Column (5): IV regression, but now including the lagged dependent variable (LDV) as a regressor

Table 6 shows that our dependent variable evolves over time in a rather erratic way for *several* municipalities. Indeed, intergovernmental *special purpose* grants to a particular municipality can be expected to be characterised by a smaller degree of persistence than the formula-based general purpose intergovernmental grants of the Municipality Fund of the Brussels Region. Nevertheless, we generally may suspect even discretionary intergovernmental grants to be characterised by an important extent of persistence/stickiness. Being (in)eligible as a municipality or (un)skillful as a political party in power in a municipality with respect to obtaining discretionary grants in year t may be explained to an important extent by being (in)eligible or (un)skillful with respect to obtaining discretionary grants in year t-1. Table 6 indeed shows that our dependent variable evolves over time in a rather smooth way for *several other* municipalities. Also, when we conduct the Wooldridge test for autocorrelation of error terms in panel data, we have to strongly reject that there would not be first order autocorrelation in the error term (p value 0.00). However, municipal eligibility or party-specific skill with respect to obtaining grants should be expected to be a rather time-invariant unobservable. Therefore it should be captured by the municipality dummies we have included so far into our regression specifications.

[254] Because of multicollinearity with year dummies, the "MRsubsgovt" variable drops out altogether in column (3) of table 12.

[255] We can calculate the Hansen J statistic because the regression behind column (4) uses more instruments than that there are instrumented variables. This is because we additionally include the "anticoalition" dummies as instruments, as shown in the full regression equation in section 5.3 above.

Still, adding an LDV as a regressor may make sense, because including an LDV takes into account the possibility that a past *increase (decrease)* in grants may cause a current *increase (decrease)* in grants. I.e. it may capture a possible *trend* in grants received not captured by municipality dummies. Therefore, as an alternative to the IV regression without an LDV, we now include the LDV into our IV regression. However, as is well known that the LDV is endogenous, we need to instrument it. We instrument the LDV with its 2nd and 3rd lag, as is common practice. However, as is also well known, for those 2nd and 3rd lags expressed in levels to be valid instruments, we should first express the original regression specification in first differences rather than including municipality dummies (the latter being equivalent to conducting a FE estimation). Therefore, before conducting an IV regression including the LDV, we conduct a First Difference transformation (shown in table 12 by "D.").

As opposed to our IV regression without an LDV in column (4), the Hansen J test now *does* allow us to reject the hypothesis that our instruments would be endogenous, with a p value well over 0.1. Somewhat surprisingly, the coefficient of the LDV is insignificant. The fact that our dependent variable "discretionary grants" appears to evolve quite erratically over time for some municipalities may explain that the coefficient of our LDV is insignificant. However, when (1) including this LDV, (2) instrumenting it -which makes us lose 3 years of observations[256]-, as well as (3) conducting the First Difference transformation rather than adding municipality dummies, the sign, size and significance of our variables of interest change remarkably compared to column (4):

- the MR coefficient changes from positive and insignificant to negative and insignificant
- the CDH coefficient becomes large but remains insignificant
- the Ecolo coefficient of interest stays positive and significant and increases in size; However, the coefficient of the "mungovt" dummy for Ecolo increases even more in size[257]; the significance and sign of the Ecolo "mungovt" and the Ecolo alignment coefficients are robust against excluding any single municipality from our sample, except that the p value of the Ecolo alignment coefficient increases to 0.16 if we omit the municipality of Sint-Agatha-Berchem
- the PS coefficient increases in size but changes from significant to just not significant anymore (p value 0.11)

An overall increase in size of our coefficients of interest compared to the IV regression without an LDV is surprising, because our coefficients of interest now only measure the *short-term* effect of a change in alignment, as opposed to column (4). It is also puzzling that the increase in coefficients appears to be caused even more by the First Difference transformation than by the addition of the LDV.

Similarly to column (4), to be able to compare with alignment effects found in the partisan alignment literature, we calculate our *average* effect of partisan alignment over our sample. For Ecolo this average effect amounts to 92.4 euros –i.e. {95.8*(170/255)}+{(95.8*2)*(38/255)}- (with a standard deviation of 42.7 euros and a p value of 0.03). In words: 92.4 euros is the sum of 95.8 –the coefficient of the Ecolo alignment variable in column (5)- weighted for the relative frequency of Ecolo being in power at *1* subsidising level- and 95.8*2 –double the coefficient of the Ecolo alignment variable in column (5)- weighted for the relative frequency of Ecolo simultaneously being in power at *2* subsidising levels. The

[256] I.e. 57 (=3*19) observations.

[257] The coefficient of the Ecolo "mungovt" dummy even becomes very large when compared to the average per capita discretionary grant amount of 216.7 euros that a Brussels municipality annually receives.

average amount of 92.4 euros is about twice the effect of 44.8 euros of column (4) and is no less than 42.6% of the annual per capita grant amount of 216.7 real euros of an average municipality. For PS the average alignment effect is obviously 19.8 euros (though just not significant at the 0.1 level), or 9.1% of the annual average per capita grant amount of 216.7 real euros[258].

Similarly to the coefficients of column (4), the coefficients of column (5) enable us to calculate the *average effect of a municipality taking Ecolo into its coalition on grants received.* The calculation of this effect is shown in table 14, and is fully analogous to the calculation in table 13. The conclusion from table 14 –in contrast to the conclusion from table 13- is that in the short run the extra grants received thanks to Ecolo alignment *by far* do not seem to suffice to compensate for the decrease in grants due to non-aligned municipal coalition participation of Ecolo. *"On a net basis"* municipalities in which Ecolo is in power as a consequence receive 110.4 euros less grants.

Table 14: Average *short-term* effect of a municipality taking Ecolo into its coalition on discretionary grants (in euro per capita) received over our sample period, regression with LDV

	(0) average alignment effect in isolation	(1) total effect if "ECOLOsubsgovt" = 0	(2) total effect if "ECOLOsubsgovt" = 1	(3) total effect if "ECOLOsubsgovt" = 2	(4) average total effect
coefficient	**92.4**	**-202.8**	**-107.0**	-11.2	**-110.4**
standard error	**42.7**	**64.24**	**55.2**	76.7	**54.9**
p value	**0.03**	**0.01**	**0.05**	0.88	**0.04**
sample share	255/255	47/255	170/255	38/255	255/255

Source: own calculations

Column (5) is our preferred regression specification, not only because it shows that the instruments used are valid and relevant, but also because it is the most general specification, including an LDV.

Column (6): IV regression, with municipal coalition participation weighted for vote share

The specification behind column (6) is an attempt to explain the positively significant and large coefficient of interest in particular for Ecolo in columns (4) and (5) , as it has been demonstrated that in some cases small parties are able to obtain extra "spoils of office" (see Browne and Franklin 1973). We cannot *directly* measure if the Ecolo alignment effect that we have found is due to the small size of Ecolo, as Ecolo is smallest party in *every* municipal and subsidising coalition of which it was part over our sample period, except one. However, we can *indirectly* measure the effect of smallness on the partisan allocation of grants in general by attaching more weight to the municipal coalition participation of large parties in the IV regression behind column (5). This is to check if grants are more strongly associated with small coalition parties than with large coalition parties. In the specification behind column (6) we have therefore *multiplied the municipal coalition participation dummies with the respective municipal vote shares.*

[258] Again keeping in mind that we should perhaps even refrain from deriving *any interpretation at all* from the PS coefficient, as we are not able to include the constitutive factors of the PS alignment variable as separate regressors.

Of course when multiplying our variables of interest with the municipal vote share concerned (expressed as a percentage), our coefficients of interest become much smaller, and it is hard to compare their sizes to the sizes of our coefficients of interest in the preceding columns. Our coefficients of interest now measure the grants increase in euro for every percentage point increase in the municipal vote share of a party that both participates in the municipal coalition and in one or more coalitions at the subsidising levels. The effect of this multiplication is that the PS coefficient again turns significant at the 0.1 level, and that the Ecolo coefficient is now significant again at the 0.01 level. The conclusion hence is that large rather than small coalition parties cause aligned municipalities to receive more grants, which does not allow us to explain the strongly positive Ecolo alignment effect on grants received with the small size of Ecolo.

We obtain a similar result when multiplying coalition participation at the *subsidising* level(s) with a party's vote share at the *subsidising* level(s) (not shown in table 12) – rather than multiplying *municipal* coalition participation with the *municipal* vote share. Considering party size at the subsidising level rather than at the municipal level may be relevant if one believes that it is the former level rather than the latter level that is the main actor with respect to partisan allocation of grants. In effect, Rodden and Wilkinson (2004) and Khemani (2007) find that Indian parties that are small resp. dominant at the *central* level -i.e. at the subsidising level– obtain more grants for "their" state in case of partisan alignment. However, similarly to weighting for *municipal* vote share, weighting for *subsidising level* vote share renders those coefficients of interest that are (close to) significant in column (5) –i.e. those related to Ecolo and PS- only *more* significant (not shown in table 12). This suggests that neither Ecolo's smallness when in power at the subsidising level(s) explains the significantly positive Ecolo alignment effect of column (5).

Column (7): IV regression, but with municipal coalition participation weighted for coalition formation power index

As said above, the political economics literature points at yet another reason why small parties may be associated more strongly than large parties with the partisan allocation of grants. It is the uncertainty argument of Persson and Tabellini (2000 p. 345-351): the more a governing party is *uncertain* that it will still be in power after the next elections, the more it will overspend in the current legislature by means of running a budget deficit. Hence we hypothesise that governing parties at the *municipal* level that are less certain of remaining in power will try to increase spending in terms of lobbying for an increase in grants allocated by friendly governments.

As said above, it is hard to distinguish preferences for certain coalitions at the Brussels municipal level over our sample period. This lack of preference patterns induces us to operationalise the degree of a particular party's certainty of remaining in power after the *next* elections not simply by means of the number of years during which a particular party was *previously* in power[259], but instead by means of its *current* Banzhaf *power index* (or coalition formation power index). As said above, the latter is the number of coalitions in which a particular party is decisive for a majority (given the most recent election results) -i.e. in which this party is *needed*- expressed as a share of all possible coalitions that command a majority

[259] E.g. if like in the 20th century at the federal level, both liberals and socialists showed a clear preference to form a coalition with the Christian-democrats, the latter's certainty of remaining in power could be operationalised as the number of years the Christian-democrats had previously been in power.
In his study on Swedish municipalities, Pettersson-Lidbom (2001) operationalises certainty of remaining in power as a dummy measuring if the party in question remained in power *ex post*, but Pettersson-Lidbom (2001) acknowledges various problems with this operationalisation, which induce him to develop a complex IV approach.

of the votes (Banzhaf 1968). We hence assume that the less a party that is part of a municipal coalition is currently needed for a municipal coalition to command a majority of the votes, the less this party is certain to remain in power after the next elections[260], and therefore the more this party will (try to) receive grants if it is also in power at one of the subsidising levels. Table 15 shows average municipal power indices for the 4 parties in question that we calculated per municipality and legislature. It -unsurprisingly- reveals that Ecolo on average has the lowest power index (but that CDH's power index is almost as low).

Table 15: Municipal power indices (in cases in which parties are part of a municipal coalition)

		MR	PS	CDH	Ecolo
Power index	Mean	72.4	54.6	31.7	31.6
	Standard deviation	*36.5*	*40.4*	*40.0*	*25.6*

Sources: http://www.ibzdgip.fgov.be/result/nl/main.html, http://www.bisa.irisnet.be/themas/verkiezingen#.UqXcgDsjJsQ, own calculations.

In column (7) of table 12 we test the possibility that the rather strong impact of Ecolo alignment on grants may be due to a low Ecolo power index –and therefore a high uncertainty of remaining in power. To this effect, we replace the municipal vote shares in column (6) as weights with the municipal power indices. The coefficients of interest in column (7) show that the regression specification with weighting for coalition formation power fits *less well* the partisan alignment hypothesis for Ecolo but fits this hypothesis *better* for PS, compared to the unweighted specification in column (5) and the specification in column (6) weighted for vote share[261]. We are hence not able to conclude the existence of any causal relationship between coalition formation power and certainty of remaining in power on the one hand and aligned allocation of grants on the other hand[262]. Such inconclusiveness may imply that a high power index may measure –rather than or next to high certainty of remaining in power, and hence less need of receiving extra grants- strong power in terms of lobbying aligned subsidising levels for extra grants. Such inconclusiveness may also be due to the strong correlation between a party's power index and a party's vote share. Effectively, table 16 –unsurprisingly- points at the strong positive correlation between vote shares and power indices, with Ecolo showing the weakest correlation[263]. In column (6) of table 12, we had already noted a positive association between a party's municipal vote share and grants provided by the aligned subsidising level.

[260] We hence assume that a party in power measures its certainty of remaining in power by its current power index.
[261] As with column (6), we cannot readily compare the *size* of the coefficients between columns (5) and (7), because of our multiplication with the power indices. The Banzhaf power index takes a value between 0 and 100%.
[262] When instead weighting being in power at the *subsidising* level(s) with the respective power indices at the *subsidising* level(s) (regression results not shown), none of the coefficients of interest remains significant.
[263] Unsurprisingly, table 16 shows that for all parties the average power index (and its standard deviation) is about twice as high as their average vote share (and its standard deviation). This is because a party may well be decisive in *100%* of all possible coalitions even when having collected *less than 50%* -but still a very high share- of the vote.

Table 16: Relationship between municipal vote shares and municipal power indices (in cases in which parties are part of a municipal coalition)

		MR	PS	CDH	Ecolo
Vote share	Mean	37.6	26.6	12.9	16.0
	Standard deviation	*15.6*	*14.3*	*9.6*	*5.4*
Power index	Mean	72.4	54.6	31.7	31.6
	Standard deviation	*36.5*	*40.4*	*40.0*	*25.6*
Correlation		0.83	0.91	0.83	**0.37**

Sources: http://www.ibzdgip.fgov.be/result/nl/main.html, http://www.bisa.irisnet.be/themas/verkiezingen#.UqXcgDsjJsQ, own calculations.

Column (8): IV regression, distinguishing between municipalities aligned with Ecolo at the Federal level and municipalities aligned with Ecolo at the Brussels Regional level

Finally we explore if any of the different subsidising levels is the "driving force" behind the Ecolo alignment effect. We can check this more easily for Ecolo because over our sample period Ecolo's participations to the various subsidising governments have overlapped less than other parties' participations (see table 7). To this effect, we consecutively interact the Ecolo municipal coalition participation dummy with 3 dummies, each taking the value of 1 only when Ecolo participates in the coalition at the Federal, Brussels Regional resp. French Community level, as shown in column (8)[264].

It appears that it is Ecolo's participation to the *Brussels Regional* government coalition that drives the effects, as the only Ecolo alignment interaction that shows a significantly positive coefficient (with a p value of 0.06) in column (8) is the one for the Brussels Regional government. This does not surprise:

- as we suspect that it is the Brussels Regional government that provides most of the grants to Brussels municipalities,
- as Ecolo is stronger in the Brussels Region than in the whole of Francophone Belgium,
- as the Brussels Regional government is the most intertwined one with Brussels municipalities,
- and as Ecolo's participations to the Brussels Regional government coalitions over our sample period were its 2nd and its 3rd at a subsidising level, so it may have undergone a learning effect

The above finding that the Ecolo participation to the Brussels Regional government from 2004 onwards may drive the Ecolo alignment effect additionally suggests that Ecolo when in power at the subsidising level(s) may well have deliberately attempted to *compensate* for Ecolo municipalities having been punished with less grants whenever Ecolo was *not* in power at any of the subsidising levels. This is because Ecolo's participation in the Brussels Regional government *succeeds* all of Ecolo's participation in municipal coalitions without Ecolo being in power at any of the subsidising levels.

However, the differences of the coefficients of the 3 Ecolo alignment interactions are not statistically significant.

[264] When we also interact the MR and CDH municipal coalition participation dummies with 3 dummies, each taking the value of 1 only when MR resp. CDH participate in the coalition at the Federal, Brussels Regional resp. French Community level, none of the interacted variables is statistically significant.

8. Conclusion

The conclusion from this paper is that the subsidising levels of Brussels municipalities do not favour those municipalities with extra grants where *voters* are aligned with the subsidising levels –as predicted by the standard partisan hypothesis-, but rather favour those municipalities with extra grants where *political parties in power* are aligned with the subsidising levels – as predicted by the partisan alignment hypothesis. Hence grants are not so much politicised in that they are *directly* oriented to sympathetic voters, but in that they at most are only *indirectly* oriented to sympathetic voters, via the favouring of sympathetic municipal governments.

We find that a number of *individual* parties that are part of the governing coalition at the subsidising level(s) are able and willing to steer grants to municipal governments that are aligned with those *individual* parties. This finding implies that *individual parties within a municipal coalition* would be able to claim the credit for (the public goods provided thanks to) grants received by a municipality. However, we find that this conclusion only holds for 2 out of the 4 major political parties active at the Brussels municipal level (Ecolo and PS), and that for 1 of those 2 political parties (PS) the effect is rather small.

The above conclusion assumes that the subsidising levels rather than the municipal levels are the "acting" levels within "the game of aligned allocation of grants". However, as argued already above, the *municipalities themselves* could be the major player, rather than the subsidising governments, while we failed to provide evidence in favour of one or of the other of these 2 possibilities.

A major difference between most of the empirical literature on the partisan alignment of grants and our research is not only that our paper investigates –and in a number of cases finds- alignment effects of *individual* political parties rather than of party blocks, but also that our paper investigates alignment effects for *junior* coalition partners compared to *dominant* coalition partners. In contrast, the RDD used by the current literature on partisan alignment only allows to investigate alignment effects of the dominant party in the coalition or of the coalition as a whole.

We find rather surprisingly that partisan alignment of municipalities with the smaller party Ecolo at their subsidising levels generates more discretionary grants than alignment with respect to the larger parties MR and PS. While asymmetric effects have been found in other countries too, the consistently significant and positive Ecolo alignment effect is surprising for 2 reasons: 1) Ecolo is the smallest of our 4 parties studied, and also the party that has been least in power over our sample period; 2) most Ecolo coalition participations at the municipal as well as at the various subsidising levels over our sample period were Ecolo's 1st ever, i.e. inexperienced ones.

Again in contrast to several other studies on the effect of partisan alignment on grants, we have obtained suggestive evidence about the reason *why* alignment of a municipality with a particular party –in our case Ecolo- generates more grants than with another party. An important motivation for Ecolo to request extra grants may have been that municipalities governed by Ecolo while Ecolo is *not* governing at any of the subsidising levels were punished with less grants by its competitor parties at the subsidising levels. Indeed, we find that municipalities in which Ecolo participates in the coalition received significantly *less* grants whenever Ecolo was *not* part of any of the subsidising government coalitions. In this way, the established parties may have tried to build a "barrier of entry" against a relative newcomer such as Ecolo on the "political market". Hence the *positive* Ecolo alignment effect on grants we find could merely be an

attempt to compensate for an initial *negative* Ecolo alignment effect on grants, i.e. for the decrease in grants that municipalities governed by Ecolo experience if Ecolo is not in power at any of the subsidising levels. We even find that the positive Ecolo alignment effect on grants does not suffice to cancel out the negative effect of Ecolo participating in municipal government without it participating in a subsidising government. We were able to come to this result because –in contrast to most other studies- we have modelled our alignment variable as an interaction variable, consisting of the multiplication of a dummy variable measuring municipal power and a count variable measuring power at the subsidising level.

We have also explored a number of other possible explanations why of our 4 parties alignment of a municipality with Ecolo generates the most extra discretionary grants. Firstly, we have explored whether the "Ecolo bonus" could be due to the sheer *smallness* of Ecolo, as it has been demonstrated elsewhere that large parties find it relatively easier to compensate small parties with extra "spoils of office" (see Browne and Franklin 1973). From this perspective, the strong relationship we find between Ecolo alignment and discretionary grants would be an indicator of potential *weakness* of Ecolo, i.e. of the possibility that discretionary grants would be one of the few "levers of power" on which it has to rely, and which Ecolo therefore was obliged to use particularly efficiently[265]. Conversely, a larger party may derive other –perhaps more important but less visible- benefits than extra discretionary grants from holding office, such as a more exclusive control over public policy in general[266]. However, we have not found empirical evidence for the "smallness bonus" explanation, neither when considering party size at the municipal level nor when considering party size at the subsidising level(s).

A 2nd explanation relates to the well-known uncertainty argument of Persson and Tabellini (2000 p. 345-351): the more a governing party is *uncertain* of its hold on power after the next elections, the more it will overspend in the current legislature. Table 4 above shows that Ecolo's municipal coalition participation was the least frequent and the most volatile of the 4 largest Brussels parties over our sample period. This infrequency and volatility may have provided ex ante an extra motivation for Ecolo to bargain for extra grants to municipalities in which it was in power, once it was also in power at one of the subsidising levels. We measured uncertainty of remaining in power with the Banzhaf power index, which measures the number of coalitions in which a party is needed with a view to obtaining a majority (as a share of total possible coalitions). This power index appears on average not particularly high for Ecolo in cases in which it was part of a municipal coalition[267]. Moreover, when weighting our municipal coalition participation dummies for the respective parties' power indices, our Ecolo coefficients of interest become less instead of more significant.

---

[265] This is reminiscent of Browne and Franklin (1973 p. 468)'s finding that small coalition parties that receive "bonus ministries" also happen to receive a disproportionate quantity of *less valued* ministries.

[266] We were able to find the following grants-related example of such more general advantages of holding office at the Brussels Regional level: PS has been a major coalition party in the government of the Brussels Region *ever since* the establishment of the Brussels Region in 1989. In 1994, i.e. during the 1st ever legislature of the Brussels Region government (1989-1995), PS reformed the Brussels Municipality Fund. At that time PS was the largest governing party at the Brussels Region level and provided the prime minister, who was also in charge of local government, while PRL, the main component of MR, did not take part in this government and opposed the reform. PS changed the allocation formula of the Municipality Fund so that poorer municipalities –concentrated in the west and the centre of the Brussels Region and traditionally dominated by PS- benefited more from it, at the expense of the richer, south-eastern and MR dominated municipalities. See also De Bock (2010).

[267] This is not surprising as Ecolo was a junior coalition partner in most cases in which it was part of a coalition.

A 3rd and most promising explanation for the strong Ecolo effect may be an inverse –and non-linear- experience effect. Paradoxically, it may precisely be *because* over our entire sample period Ecolo was relatively new in power –most cases of Ecolo coalition participation being its 1st or 2nd ever both at the municipal and at the subsidising level-, and hence because Ecolo was relatively inexperienced, that it *initially* obtained less grants as a municipal coalition partner, notably when it was *not* also governing at one of the subsidising levels, *and* that it *subsequently* managed to obtain more grants once it obtained power both at the municipal and the subsidising levels. I.e. its inexperience may have meant that it was particularly *motivated* to lobby -either within the subsidising coalition, within the municipal coalition, or from the municipal to the subsidising level- for extra grants to municipalities in which it was in power, possibly to compensate the initial reduction in grants it had suffered. Of course we have not been able to test this possibility as Ecolo was relatively new in power in municipal as well as subsidising governments concerned over our *entire* sample period.

We end our conclusion with 2 lines of caution against the results we obtained. *Firstly*, with respect to PS a complication is that we are not sure if we can ascribe the effect we have found to alignment, as there is no variation in PS coalition participation at the subsidising levels over our sample period. The only PS variation is in coalition participation at the municipal level. Hence the PS effect could be attributable to skill, experience and/or motivation of local PS politicians to obtain more grants. We should perhaps even refrain from deriving *any interpretation at all* from the PS coefficient, as we were forced to omit the constitutive factors of an PS alignment variable as separate regressors from our regression equation. However, because of the little variation in PS alignment, it could be rather surprising that we have still found a PS effect in some of our specifications in the first place. Should we have been able to exploit variation in PS coalition participation at the subsidising level(s), we perhaps would have found a larger and more significant effect of alignment with respect to PS than the effect we have found in this paper.

A *2nd* handicap our analysis had to deal with from the start is that our data are not conducive to ascribing the alignment effects found to a particular subsidising level. This is because we had to face the important disadvantage -compared to e.g. the similar study for Spain by Curto-Grau e.a. (2012)- that our data generally do not allow us to distinguish between Federal, Regional, and Community grants to the Brussels municipalities.

References

Albouy, D., "Partisan representation in Congress and the geographic distribution of federal funds", Review of Economics and Statistics 95.1 (2013): 127-141.

Allers, M, and E. Merkus, "Soft budget constraint but no moral hazard? The Dutch local government bailout puzzle". University of Groningen working paper, 2013.

Ansolabehere, S., J. Snyder, A. Strauss, and M. Ting, "Voting weights and formateur advantages in the formation of coalition governments", American Journal of Political Science 49.3 (2005): 550-563.

Arcq, E., P. Blaise and V. de Coorebyter, « Les résultats des élections communales du 8 octobre 2000 à Bruxelles », Courrier hebdomadaire du CRISP 1742-1743 (2001) : 57-87.

Arulampalam, W., S. Dasgupta, A. Dhillon, and B. Dutta, “Electoral goals and center-state transfers. A theoretical model and empirical evidence from India”, Journal of Development Economics 88.1 (2009): 103-119.

Banzhaf, J., “One man, 3.12… votes: a mathematical analysis of the electoral college”, Villanova Law Review 13 (1968): 304-346.

Baron, D. and J. Ferejohn, “Bargaining in legislatures”, American Political Science Review 83.4 (1989): 1181-1206..

Baskaran, T. and Z. Hessami, “Political alignment and intergovernmental transfers in parliamentary systems”. Working paper, 2014.

Belfius, “Lokale financiën. Gemeenten en provincies”, 2010.

Belgisch Staatsblad, “Ordonnantie tot vaststelling van de regels voor de verdeling voor de algemene dotatie aan de gemeenten van het Brussels Hoofdstedelijk Gewest vanaf het jaar 1998”, 1998.

Biswas, R., S. Marjit, and V. Marimoutou, “Fiscal federalism, state lobbying and discretionary finance. Evidence from India”, Economics & Politics 22 (2010): 68-91.

Bordignon, M., M. Gamalerio, and G. Turati, “Decentralisation, Vertical Fiscal Imbalance, and Political Selection”. Working paper, 2013.

Bracco, E. and A. Brugnoli, “Unequal treatment of equal municipalities. The effect of local institutions on intergovernmental relationships”. Working Paper, 2014.

Bracco, E., F. Porcelli, and M. Redoano, “Incumbent effects and partisan alignment in local elections: a regression discontinuity analysis using Italian data”. Working paper, 2012.

Brambor, T., W. Clark and M. Golder, "Understanding interaction models: Improving empirical analyses", Political Analysis 14.1 (2005): 63-82.

Brollo, F. and T. Nannicini, “Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil”, American Political Science Review 106 (2012): 742-761.

Brollo, F., T. Nannicini, R. Perotti and G. Tabellini, “The Political Resource Curse”, American Economic Review 103.5 (2013): 1759-1796.

Browne, E. and M. Franklin, “Aspects of coalition payoffs in European parliamentary democracies”, American Political Science Review 67.2 (1973): 453-469.

Cattoir, P., J. Vaesen, M. Verdonck, G. Van der Stichele, and P. Zimmer, “Finances publiques à Bruxelles. Analyse et enjeux”, Courrier hebdomadaire du CRISP, 2007-2008 (2009): 5-61.

Curto-Grau, M., A. Solé-Ollé and P. Sorribas-Navarro, “Partisan targeting of intergovernmental transfers and state interference in local elections. Evidence from Spain”. Working paper, 2012.

Dahlberg, M. et al., "Using a discontinuous grant rule to identify the effect of grants on local taxes and spending", Journal of Public Economics 92 (2008) 2320-2335.

Dasgupta, S., A. Dhillon, and B. Dutta, "Electoral goals and centre-state transfers. A theoretical model and empirical evidence from India". University of Warwick, unpublished manuscript, 2004.

De Bock, E., « 20 ans de Dotation Générale aux Communes », 2010.

de Coorebyter, V., « Les résultats des élections communales du 8 octobre 2006 à Bruxelles », Courrier hebdomadaire du CRISP, 1980 (2007): 7-50.

De Morgen, « De Donnéa gewipt als burgemeester », October 10 2000.

Engstrom, E. and G. Vanberg, "Assessing the allocation of pork. Evidence from Congressional earmarks", American Politics Research 38.6 (2010): 959:985.

Erikson, R., O. Folke, and J. Snyder "A gubernatorial helping hand? How governors affect presidential elections". Working paper, 2014.

Fiva, J. and A. Halse, "Unconditional love? Pork barrel politics in the absence of electoral incentives". Working paper, 2014.

Folke, O., "Shades of brown and green: party effects in proportional election systems". Working paper, 2013.

Golden, M. and L. Picci, "Pork-barrel politics in postwar Italy, 1953-94", American Journal of Political Science 52.2 (2008): 268-289.

Jennes, G., « Has Belgian fiscal decentralisation reduced the size of government and the budget deficit ? ». Vives discussion paper 42, 2014.

Jennes, G. and D. Persyn, "The effect of political representation on the geographic income distribution in Belgium: evidence using Belgian data", European Journal of Political Economy 37 (2015): 178-194.

Khemani, S., "Party politics and fiscal discipline in a federation: Evidence from the states of India", Comparative political studies 40 (2007): 691-712.

Knight, B., "Estimating the value of proposal power", American Economic Review 95 (2005): 1639-1652.

Knight, B., « Legislative representation, bargaining power and the distribution of federal funds : Evidence from the US Congress », Economic Journal 118 (2008) : 1785-1803.

Larcinese, V. L. Riszo, and C. Testa, "Allocating the US federal budget to the states: the impact of the president", Journal of Politics 68 (2006): 447-456.

Laver, M. and K. Shepsle, « Making and breaking governments. Cabinets and legislatures in parliamentary democracies ». Cambridge University Press, 1996.

Laver, M. and K. Shepsle, «Ministrables and government formation », Journal of Theoretical Politics 12.1 (2000): 113-124.

Lentzen, E. and X. Mabille, « Les résultats des élections communales du 9 octobre 1994 », Courrier hebdomadaire du CRISP 1457-1458 (1994).

Le Vif/L'Express, "A couteaux tirés", October 6, 2006.

Le Vif/L'Express, "La part du lion des subsides", July 1, 2011.

Le Vif/L'Express, "Le scandale des subsides", October 5, 2012.

Martin, M. and G. Vanberg, "Wasting time? The impact of ideology on size and delay in coalition formation", British Journal of Political Science 33 (2003): 323-244.

Migueis, M., "The effect of political alignment on transfers to Portuguese municipalities", Economics & Politics 25 (2013):110-133.

Milligan, K. and M. Smart, "Regional grants as pork barrel politics", CESifo working paper no. 1453, 2005.

Nassaux, J.-P., « La formation des coalitions dans les communes bruxelloises », Courrier hebdomadaire du CRISP 1770 (2002): 5-38.

Rodden, J. and S. Wilkinson, "The shifting political economy of redistribution in the Indian Federation". Preliminary draft, 2004.

Rodden, J., "Hamilton's Paradox: The Promise and Peril of Fiscal Federalism". MIT, 2005.

Ryan, M., "Legislative tenure, federal spending, and state economic performance". Working paper, 2008.

Schelker, M., "Lame Ducks and Divided Government: How Voters Control the Unaccountable". CESifo Working Paper Nr. 3523, 2011.

Schofield, N. and M. Laver, "Bargaining theory and portfolio payoffs", British Journal of Political Science 15 (1985): 143-164.

Solé-Ollé, A. and P. Sorribas-Navarro, "The effects of partisan alignment on the allocation of intergovernmental transfers. Differences-in-differences estimates for Spain", Journal of Public Economics 92.12 (2008): 2302-2319.

Sørensen, R., "The political economy of intergovernmental grants: the Norwegian case", European Journal of Political Research 42 (2003): 163-195.

Veiga, L. and F. Veiga (2013), "Intergovernmental fiscal transfers as pork barrel", Public choice 155.3-4 (2013): 335-353.

Chapter 5: A test of the Law of 1/n for the supra-local governments of Belgium: does "more politicians" mean "more public spending" or "a larger budget deficit"?[268]

Abstract

We investigate if a more populous or fragmented executive or legislative increases public spending and/or the budget deficit, an effect known as the Law of 1/n. We test this relationship for the supra-local governments of Belgium. We find few indications that more politicians or more fragmented governments –either in terms of a larger or more fragmented executive or in terms of a larger or more fragmented legislative- lead to an increase in overall public expenditures or in the budget deficit. The exception is that we find a significantly positive effect of a change in the size of the *executive*, more in particular of the number of ministers composing the governing coalition, on the budget deficit. Indeed, politicians may not only try and favour their constituency with targeted extra spending but also with targeted tax cuts, in turn increasing the budget deficit.

Our identification is based on an IV regression approach, instrumenting the number of ministers overall with their resp. number in governments in charge of a decentralisation round. Belgian governments in charge of a decentralisation round are constitutionally obliged to rely on broad parliamentary support, and therefore tend to be larger.

Keywords: distributive politics; public expenditures; politicians; political institutions

[268] I thank Thushyanthan Baskaran, Erik Buyst, Danny Geerts, Joep Konings, Bart Maddens, Klaas Staal, David Stadelmann, Robrecht Vandendriessche, Tom Van Puyenbroeck, Jakob Vanschoonbeek, Francisco Veiga, and participants at the 2015 IIPF conference in Dublin for advice on the empirical strategy. For data provision, I thank Christophe Van Gheluwe (www.cumuleo.be), Bart Maddens and Jef Smulders from the KULeuven Department of Political Science, Statistics Belgium, the Federal Ministry of the Budget, the Federal Ministry of Finance, the Federal Ministry of Employment, and the various statistical agencies, budget departments and/or study departments of the governments of:

- the Brussels Region (BISA),
- the Flemish Region/Community (Studiedienst van de Vlaamse Regering),
- the French Community (Direction Générale du Budget et des Finances),
- the German Speaking Community (Ministerium und Parlament der Deutschsprachigen Gemeinschaft),
- and the Walloon Region (IWEPS)

1. Introduction

In 1981, Weingast e.a. predicted in a seminal theoretical paper that government spending would increase in the number of electoral districts. Weingast e.a. (1981) demonstrated that the smaller the share of a single electoral district in total tax revenue raised, the less a politician elected in that district would internalise the cost of taxation, hence increasing his perceived net benefit of government spending in his district. The share of a single electoral district in total tax revenue is 1/n, assuming that there are n districts of equal size. Additional assumptions made by Weingast e.a. (1981) are that the parliament and only the parliament decides on expenditure, and that each district elects only one politician, i.e. that the electoral system is not a proportional but a majoritarian one. Moreover, the more powerful each single district is within the decision making process, the worse the common pool problem becomes. In the extreme, each district is a "veto player", i.e. each district's consent is needed for legislation to pass. Weingast e.a. (1981)'s prediction has since then been known as the the Law of 1/n, and as the common pool problem.

However, Primo and Snyder (2008 p. 477) point out that since the publication of Weingast e.a.'s seminal paper, the Law of 1/n has typically been interpreted as linking *the number of decision makers in general* to public spending: "The logic is powerful and can be used to understand many political phenomena beyond the impact of legislature size on total spending, including why multiparty governments might be expected to spend more than single-party governments, and related, why proportional-representation electoral systems might lead to more spending than majoritarian systems. The idea of a ''common pool'' problem worsening as the number of decision makers increases permeates many questions in political science, so much so that the result is often stated as a given in the literature."

In what follows, we will develop a test according to such a more general interpretation of the Law of 1/n, based on data collected with respect to Belgian supra-local governments. Our contribution to the literature will be that we will not only interpret "n" according to its narrow, literal meaning –i.e. the number of geographic electoral districts, typically in a majoritarian electoral system-, but rather more generally as the number of decision-makers involved in fiscal policy, *both* within the legislative and within the executive, and irrespective of the electoral system. Moreover, we will interpret "n" both in terms of *geographically based* interest groups ("electoral districts") and in terms of interest groups *with a social or bureaucratic basis* ("constituencies")[269]. This is also because most Belgian politicians not only have a geographic constituency but also a social or bureaucratic constituency. That most Belgian politicians do not seem to focus their re-election efforts exclusively on their geographic constituency is partly due to Belgium's proportional electoral system, in which any electoral district elects more than one politician[270], and in which credit claiming for geographically targeted spending is hence rather difficult.

In what follows, we will even give more attention to the executive than to the legislative. This is because our hypothesis is that any positive effect of the size of the *executive* on overall expenditure and/or on the

[269] One could however imagine other reasons than *re-election* for a politician to increase expenditures. Referring to Niskanen (1971), politicians seem to share characteristics with civil servants, in that both seem to have an interest in expenditure maximisation *as such*. Public spending would also seem to be the "natural behaviour" even of any benevolent, non-reelection driven politician, as public spending seems to be a politician's natural reaction to social problems.

[270] E.g. since the 2002 Federal electoral districts merger, the smallest Federal electoral district (Luxembourg) elects 4 members of the House of Representatives.

budget deficit will likely be larger than any positive effect of the size of the *legislative* on overall expenditure and/or on the budget deficit, as in Belgium the executive is known to be more powerful than the legislative[271]. In a nutshell, this is because the career prospects of individual members of parliament (MPs) are largely determined by the political party they belong to, and because the most powerful politicians of any political party have as their major career ambition to be appointed as a minister by the president of their party[272]. For these –as well as for other- reasons[273] Belgium is labeled a "party-cracy". For the dominance of the executive and of political parties in Belgium, see e.g. Timmermans (1994) and Dewachter (2001).

In our regression analysis below, we will find few indications that more politicians or more fragmented governments lead to an increase in overall public expenditures or in the budget deficit. The exception is that we will find a statistically positive effect of a change in the size of the *executive*, more in particular of the number of ministers composing the governing coalition, on the budget deficit. The budget deficit may make sense as an alternative dependent variable instead of total spending, because politicians may not only try and favour their constituency with targeted extra spending but also with targeted tax cuts, in turn increasing the budget deficit[274]. We will also find that there is no effect of a change in the size or fragmentation of the *legislative* on public spending nor on the budget deficit.

The remainder of the paper comprises a motivation of our research, a presentation of the 9 supra-local governments of Belgium, a literature overview, a discussion of our dataset, an outline of our empirical strategy, a report of our regression results, and a conclusion.

[271] Jennes (2014a) and Jennes and Persyn (2015) are examples of empirical demonstrations that players within Belgian government executives are able to steer extra spending to their "constituencies": Jennes (2014a) demonstrates that political parties in power at government levels that subsidise municipalities of the Brussels Region are able to favour party-politically aligned municipalities. Jennes and Persyn (2015) demonstrate that Federal ministers manage to favour inhabitants of their electoral district with more social spending and/or less taxation.

[272] Or to become the president of their party themselves (or both at the same time).

[273] Another reason is that also several senior civil servants are appointed by the political parties in power (according to an informal quota system).

[274] However, of the 9 supra-local governments that we consider, only 4 have substantial competencies over tax policy: the Federal government and the 3 Regional governments. The latter only have substantial tax competencies since the "state reform" of 2001. Because 8 of the 9 supra-local governments rely on grants from the Federal government for most of their financing, one could even argue that an increase in "n" for those 8 governments may be expected to lead to an increase –rather than a decrease- in revenue. This is because those 8 governments have an interest in lobbying for an increase in grants allocated by the Federal government. The problem with this lobbying argument is that an increase in Federal grants means less revenue remaining for the Federal government, and that this increase in grants can only be implemented by means of a "state reform" voted by the Federal government. Over our sample period, only Federal grants to the 3 Community governments have been increased, i.e. on the occasion of the 1993 and 2001 "state reforms".

2. Motivation of the research question

For several reasons, Belgium seems an interesting case to test the Law of 1/n at levels of government above the local –i.e. provincial and municipal- level:

1) The supra-local level in Belgium accounts for the bulk of public expenditure. At the OECD website[275], we find that local public expenditure in Belgium amounted to 6.3 and 7.1% of GDP in resp. 1995 and 2013[276], while supra-local public expenditure in Belgium amounted to 45.7 and 47.4% of GDP in resp. 1995 and 2013, with considerably more variation in the amount of supra-local public expenditure over this time period than in the amount of local public expenditure[277].
2) Our sample period 1982-2012 covers a time period during which considerable changes took place in the "n" meant by the Law of 1/n –changes which are conducive to statistical analysis. These changes were mainly due to the "state reforms" –i.e. decentralisation rounds- of 1988-89, 1993, and 2001. Not only have new governments been established over that time period[278] –i.e. the 4 Brussels governments have been created in 1989- but also have existing governments obtained more *political autonomy* –i.e. the Flemish Region/Community, the Walloon Region, and the French Community in 1993- and more *expenditure responsibilities* –i.e. the then existing Regions and Communities in 1988-89 and 2001.

   These changes have led to changes in the "n" meant by the Law of 1/n. The various institutional changes provoked by the "state reforms" included in our sample period are not only changes in the number of electoral districts per individual supra-local government, but also changes in the number of seats in individual supra-local government's parliaments.

   Additionally, we will use the opportunity to also understand "n" as:
   - the number of ministers per individual supra-local government's governing coalition,
   - the number of parties included in each individual supra-local government's governing coalition (the 2 latter measures are measures of government fragmentation rather than measures of the number of "districts"),
   - the number of ministers' "second jobs" per individual supra-local government,
   - and the number of parties per individual supra-local government that obtained at least one parliamentary seat (see data section for more detail on these meanings of "n")

[275] See OECD fiscal federalism network database:
http://www.oecd.org/tax/federalism/oecdfiscaldecentralisationdatabase.htm#C_3

[276] Resp. the first and the most recent year for which the OECD provides data.

[277] However, as shown by table 2 below some of the Belgian supra-local governments account for much more expenditures than others. Despite successive decentralisation rounds, the by far largest Belgian government remains the Federal government. At the OECD website, we find that Federal government expenditures in Belgium amounted to 34.9 and 34.1% of GDP in resp. 1995 and 2013, while expenditures by the Belgian Regions and Communities taken together amounted to only 10.9 and 13.3% of GDP in resp. 1995 and 2013.

[278] The creation of new governments *as such* is a change in "n" that may impact on the amount of public expenditures and/or on the size of the budget deficit. Of course our panel regression analysis of Belgium is not suited to investigate this impact (because of the absence of a counterfactual).

The Belgian supra-local level provides us with an opportunity to understand "n" in many more ways than the local level[279]. Our understanding of "n" in a multitude of ways distinguishes our paper from the existing literature on the Law of 1/n.

3) Belgium is generally thought of as a country characterised by both high (supra-local) public expenditure and a high number of (supra-local) politicians. Taking the first and the most recent year available in OECD statistics, total public expenditure in Belgium amounted to 51.1 and 54.4% in 1997 resp. 2013, being the 6th highest amount of all 31 OECD countries in both years[280]. But conventional wisdom in Belgium has it that it is *the Belgian decentralisation process* –having led to an increase in *the number of (supra-local) governments* since the 1st "state reform" of 1970- that (at least partly) explains both the high level of public expenditures and the high number of politicians. Contrarily to conventional wisdom, our hypothesis is that it is *the (high) number of politicians as such* –and not (or not only) the decentralisation process that has led to the existing (high) number of politicians-, that partly explains the high level of public expenditure in Belgium. Otherwise stated: our hypothesis is that it is *the way* in which Belgium has been decentralised that partly explains the high level of public expenditures in Belgium[281], i.e.:
    - by *creating* an entire new level of government –the Regions and Communities- rather than making existing lower level governments –the provinces and the municipalities- more autonomous
    - without considerably downsizing or abolishing any other level of government –e.g. ever since the 1st "state reform" of 1970, municipalities nor provinces have been downsized in a noteworthy way[282]

Although we will test the Law of 1/n according to various of its possible meanings, a potentially important meaning of "n" of which we *choose* not to test the impact is the number of civil servants. Reminding of Niskanen (1971), Pettersson-Lidbom (2012a) states that civil servants of Swedish local governments are powerful and autonomous enough to influence the size of public expenditures. We see 3 differences between the Swedish and the Belgian context in this respect. Firstly, in contrast to Sweden, Belgium is generally assumed to be a "party-cracy", rather than a "bureau-cracy", with in particular the most powerful civil servants being appointed by political parties. Secondly, as in other countries there are many

[279] As opposed to e.g. Baskaran (2013), we hence will understand "n" not only in terms of the executive but also in terms of the legislative. Baskaran (2013 p. 357 and 368) motivates his understanding of "n" for the German states as limited to the executive with the argument that the *number of political parties* represented in the legislative has no impact on the amount of expenditures as opposed to the executive, because of which Baskaran only tests the impact of the size of the executive on the amount of expenditures. One could argue that this lack of impact is however not the case with respect to the *number of MPs*: while opposition MPs may not be able to influence the amount of expenditures, MPs supporting the government may well be able to, as the latter may have privileged access to ministers, to be able to plead in favour of more expenditures to their constituency, be it their "geographical constituency" or their "social constituency". Likewise, the number of electoral districts from which a parliament is elected may then also influence total expenditures made.

[280] See www.oecd.org.

[281] Another difference with conventional wisdom in Belgium is that our investigation focuses on the *indirect* expenditure consequences of the number of politicians. I.e. because of their incentive to get re-elected they seem to have an incentive to create extra expenditures, to be channelled to their electoral district. Conventional wisdom has it that politicians mostly *directly* cause public expenditures, i.e. because of their wage bill, their offices and other operating expenditures, and their cabinet of private advisors.

[282] Except for the major merger of municipalities conducted in 1976, but this operation should not be linked to the Belgian decentralisation process. Besides, in 1976, local government oversight was still 100% a central government competence.

more civil servants in Belgium than politicians (in the different meanings of "n"), and the public sector wage bill makes up a large part of total public spending. Therefore, much more for civil servants than for politicians[283], an increase in public expenditures could simply measure the *direct* cost of an increase in civil servants, rather than their *indirect* cost[284]. Thirdly, the number of and expenditures on civil servants may be argued to be endogenous to total public expenditure: a government wishing to increase expenditure may need an increase in the number of civil servants to this effect, or may aim at an increase in the number of civil servants –or in the public sector wage bill- as a component of the expenditure increase aimed at.

3. Institutional setting: the 9 supra-local governments of Belgium

Belgium is a federal country consisting of a Federal government, 3 Regional governments and 3 Community governments. Additional governments above the provincial and municipal level[285] are the VGC and the COCOF[286], which resp. provide public goods related to Community competencies for the Flemish resp. Francophone inhabitants of the Brussels Region[287], and the GGC[288], which provides public goods related to Community competencies concerning *all* inhabitants of the Brussels Region (i.e. care for children, handicapped, and elderly). Hence in Belgium there exist 9 governments above the local level (and not 10 as one might conclude, because immediately after the creation of the Regions and Communities in 1980, the Flemish Community and Regional government merged into one). Table 1 gives an overview of the 9 supra-local Belgian governments and their competencies. In parallel, chart 1 visualises the geographic reach of those 9 governments.

[283] We presume that the direct costs of politicians are too small compared to overall public expenditures to be "picked up" by a regression analysis of overall public expenditures on the resp. meanings of "n".

[284] Moreover, we were not able to calculate public expenditures net of wage expenditures over our sample period. This is because we were able to obtain data on spending on public sector wages by each of the 9 governments only from the year 2000 onwards –data provided by the Federal Ministry of the Budget. (The exception is the Federal government, for which the "Conjunctuurnota" of the Federal Ministry of Finance provides data on the public sector wage bill from 1980 onwards.)

[285] In Belgium, provincial governments hardly have any competencies, as opposed to municipal governments.

[286] In full: "Vlaamse Gemeenschapscommissie" and "Commission de la Communauté française".

[287] As an extra complicating factor, VGC and COCOF do not exactly mirror each other in terms of competencies. I.e. the Flemish government has "contracted out" more competencies to VGC than the French Community government has "contracted out" to COCOF. E.g. COCOF is not in charge of expenditures in the field of Francophone education on Brussels territory while VGC does have the competency over Dutch-speaking education in Brussels. This partly explains the strongly diverging average VGC and COCOF expenditure amounts shown in table 2 below. Additionally, the Flemish government traditionally aims at providing "person-related" public goods to no less than 1/3 of the Brussels population, although the number of Dutch-speaking inhabitants of Brussels is estimated to have continuously decreased over our sample period. E.g. in the 2006 Brussels municipal elections, Flemish parties campaigning on a separate list obtained less than 10% of the total vote (de Coorebyter 2007 p. 20).

[288] In full: "Gemeenschappelijke Gemeenschapscommissie".

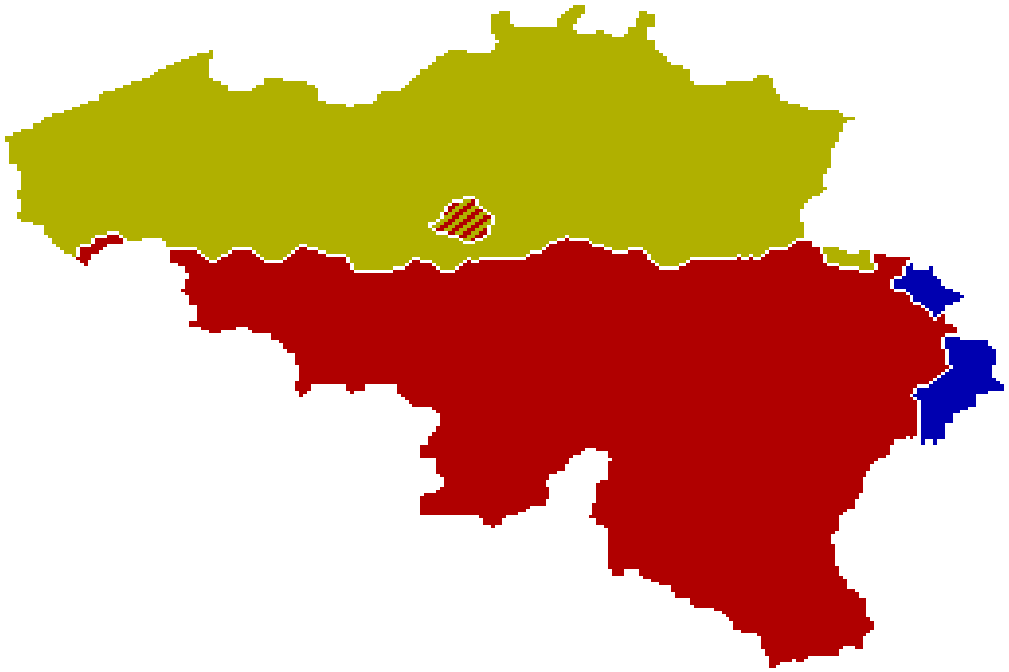
Table 1: Competencies of supra-local governments of Belgium

Supra-local government	Competencies
Federal government	Social security, justice, police, defense, …
Communities (Flemish, French, German Speaking)	"Personal" affairs (education, culture, care of children, elderly, and handicapped, …)
Regions (Flemish, Walloon, Brussels)	"Territorial" affairs (infrastructure, industrial policy, …)
Community Commissions (VGC, COCOF, GGC)	"Personal" affairs within the Brussels Region

It should be understood that, because those governments that are called "states" in other Federal countries consist in Belgium both of Regional and Community governments[289], these 2 types of government considerably overlap each other *territorially* in Belgium. Firstly, the Flemish Community government caters to the entire territory of the Flemish Region (but this does not matter as the Flemish Community government and the Flemish Regional government have been merged from the onset), while the French Community government caters to the entire territory of the Walloon Region except the territory taken care of by the German Speaking Community government. Secondly, both the Flemish (Community) government and the French Community government are active on the territory of the Brussels Region, "directly" as well as "indirectly". They are "indirectly" active because they fund VGC resp. COCOF. VGC, COCOF, and GGC, which are all 3 active on the entire territory of the Brussels Region –and only on its territory- and which are only exerting Community competencies, should be considered as governments distinct from both the Brussels Regional government as well as from other Belgian supra-local governments. While VGC and COCOF are *financed* by the Flemish government resp. the French Community government (as well as by the Brussels Regional government) (see Algoed 2009 p. 7-8 for more detail), VGC and COCOF are *governed* by ministers and MPs that do *not* belong to the Flemish government nor to the French Community government. Instead they are governed by Flemish resp. Francophone ministers and MPs of the Brussels Regional government and parliament. Likewise for GGC: while it is *financed* by both the Federal and the Brussels Regional governments, and while it is only in charge of certain Community –as opposed to Regional- competencies on the territory of the Brussels Region, it is *governed* by those same ministers and MPs who govern the Brussels Region.

[289] Decentralisation of Belgium along Community lines has always been a Flemish demand, as Flanders wished to preserve the right to offer –"person-related"- public goods to Dutch-speaking Belgians on the territory of the Brussels Region.

Chart 1: Territory of supra-local governments of Belgium



Legend:

- Green: Flemish (Regional) government
- Red + blue: Walloon Regional government
- Blue: German Speaking Community government
- Hatched: Brussels Regional government, GGC, VGC and COCOF
- Green + hatched: Flemish (Community) government
- Red + hatched: French Community government

Source: Kneiphof and Bjørklid (2010),

http://commons.wikimedia.org/wiki/File%3ABelgische_gewesten_en_gemeenschappen.png

However, in contrast to partial overlap with respect to *politicians and territory*, *expenditures and revenues* –i.e. competencies- of the 9 supra-local governments largely do *not* overlap. In other words: all 9 supra-local governments are full-fledged governments with generally speaking distinguishable competencies, revenues and expenditures.

Table 2 compares the sizes of the 9 supra-local governments of Belgium. The variation in size is very large, both in terms of population covered and in terms of expenditures, expressed either in absolute terms or per capita. Over our sample period –which is 1989-2012, except for the Federal government, for which

it is 1982-2012[290]-, the Federal government was by far the largest Belgian government, population- and expenditure-wise, mainly because social security was an exclusively Federal competency over our sample period. By far the 2nd largest government is the Flemish government, not only because inhabitants of the Flemish Region account for about 60% of total Belgian population, but also because it is the merger of the Flemish Regional and Community governments (since 1980). In Francophone Belgium, the French Community government is larger than the Walloon Regional government mainly because the former additionally caters to Francophone inhabitants of the Brussels Region. Because there are no "language counts" in Belgium, there are no data on the number of Francophone resp. Dutch speaking inhabitants of the Brussels Region. Based on the language of the political party in favour of which votes were cast during elections in the Brussels Region, and for reasons of simplicity, we assumed throughout our entire sample period that Francophone and Dutch speaking inhabitants made up 90 resp. 10% of total inhabitants of the Brussels Region[291]. It appears that 4 of the 9 governments –GGC, VGC, COCOF, and German speaking Community government- are much smaller –either population- or expenditure-wise- than the other 5 governments, a fact that we will take into account in our regression analysis (see below).

Table 2: Sizes of the supra-local governments of Belgium, in terms of *mean* population and *mean* (primary) expenditures per government over our sample period

government	population	expenditures (in 1000 real euros of 2012)	expenditures per capita (in real euros of 2012)
Federal government	10,279,561	90,876,130	8,840
Flemish government	6,009,862	20,461,750	3,405
French Community	4,282,560	8,170,165	1,908
Walloon Region	3,378,689	6,088,054	1,802
Brussels Region	1,004,301	2,218,665	2,209
GGC	1,004,301	68,921	69
COCOF	903,871	308,779	342
VGC	100,430	121,429	1,209
German Speaking Community	71,515	160,251	2,241

Sources: Statistics Belgium, "Conjunctuurnota" of the Ministry of Finance, website of the "Algemene Gegevensbank FOD B&B" (http://www.budgetFederal.be/NL/figures/Pages/databank.aspx).

However, what the 9 governments have in common is that they –and only they- are Belgian supra-local governments, with supra-local competencies, elected parliaments, and governments consisting of ministers and –in some cases- junior ministers. What they also have in common is *the Belgian context*: there is an important overlap with respect to the political culture that provides the context for the functioning of these governments: they share the electoral system, the system of party funding, and the culture of large ministerial private cabinets, of a large class of career politicians, and of the partisan appointment of

[290] Indeed, the panel dataset we have been able to establish is unbalanced: the Federal government was unable to provide us with expenditure and revenue data with respect to individual "lower-level" supra-local governments that pre-date the year 1989. Hence for the 1982-1988 period the only supra-local government for which we have expenditure and revenue data available is the Federal government (see below).

[291] As said above, e.g. in the 2006 Brussels municipal elections, Flemish parties campaigning on a separate list obtained less than 10% of the total vote (de Coorebyter 2007 p. 20).

several senior positions within the civil service. In spite of the large differences between the 9 governments –in terms of size, competencies, and set of political parties governing them[292]-, an advantage of limiting our test of the Law of 1/n to these 9 *Belgian* governments is that we avoid to some extent the problem of omitted unobserved variables that explains between-government heterogeneity in a multi-country study. We will stress this advantage when discussing our empirical strategy.

4. (Non-exhaustive) literature overview

The theoretical foundation for the Law of 1/n was already laid in 1981, when Weingast e.a. (1981) demonstrated mathematically that if taxation is country-wide and to the extent that expenditures can be targeted locally, an increase in the number of electoral districts increases the incentive of politicians to overspend on their district, as they only internalise a fraction of the tax cost of these expenditures. Weingast e.a. (1981)'s theory is to be situated in the US context. The US is characterised by a House of Representatives that is powerful in fiscal terms as well as by a majoritarian electoral system resulting in only 1 House member per electoral district. A consequence of the US context is that the number of electoral districts equals the size of the legislative (i.e. of the parliament). More districts hence means larger legislatives, while in a proportional system it would well be possible to e.g. reduce the number of districts while keeping constant –or even increasing- the number of MPs. The majoritarian US context seems to pose a bigger common pool problem than a proportional system resulting in more than one elected politicians per district. However, we hypothesise that also politicians elected in a proportional system would be subject to the common pool problem. This is a.o. things because, as Schaltegger and Feld (2009 p. 36) suggest, not only fragmentation in the legislative but also fragmentation in the executive or in the number of interest groups lobbying politicians may increase spending.

Interestingly however, Primo and Snyder (2008) demonstrate theoretically that a "*reverse* Law of 1/n" often holds for the US context: expenditures *de*crease in the number of electoral districts.

4.1. *"Direct"* empirical investigations of the Law of 1/n

A number of empirical investigations of the Law of 1/n can be considered as *"direct"* investigations of the Law of 1/n, because they give a purely *legislative* meaning to "n". I.e. these studies understand "n" either as the number of MPs or as the number of electoral districts. Moreover, as far as we know, only 1 of these investigations (Aid and Shvets 2012; see below) concerns the supra-local level[293]. Almost all "direct" empirical tests of the Law of 1/n concern US cities[294]. However, those that were conducted before 2008 do

[292] In the 1970s all major Belgian parties "communitised" into separate Francophone and Flemish wings, due to which since then virtually all parties represented in the Belgian parliaments collect votes only in one of the 3 Belgian Communities. The exception is the federal electoral district of Brussels-Halle-Vilvoorde, straddling the Brussels Region and the Flemish Region, and hence also straddling the French Community and the Flemish Community.

[293] However, some countries are so decentralised that the distinction between the supra-local and the local level becomes less relevant. An example is the sizeable expenditure responsibility, tax autonomy and political autonomy of Swedish municipalities.

[294] Interestingly, with respect to the US *federal government*, Inman and Fitts (1990) already provided some tentative evidence on the positive effect of the "size" of US Congress on expenditures (as well as on its negative effect on allocative efficiency). Inman and Rubinfeld (1997 p. 51-52) argue that the US Congress is a legislative characterised

not include fixed effects to control for unobserved causes of differences between states or cities, let alone a proper Instrumental Variables (IV) approach or Regression Discontinuity Design (RDD) to take into account endogeneity of the independent variable of interest. A notable case in point is Baqir (2002). MacDonald (2008) is the first "direct" study of the Law of 1/n that acknowledges the importance of city or state fixed effects and it finds that the positive relationship between city council size and expenditures disappears once including such fixed effects.

In contrast, Brooks e.a. (2011) find that larger council sizes lead US cities to turn a higher share of the Federal grants they receive into expenditures (as opposed to turning a part of those grants into tax cuts). The study by Brooks e.a. (2011) hence combines the empirical flypaper literature with the empirical Law of 1/n literature. Interestingly, Brooks e.a. (2011) find a way around the fact that there are no within changes in council sizes over their sample period, by interacting council size with grants, as the amounts of the latter *do change* over their sample period. However, apart from including city fixed effects, Brooks e.a. do not develop an empirical strategy that deals with the possible endogeneity of council size[295] as well as of grants. Another variation to the empirical Law of 1/n literature is the study by Aidt and Shvets (2012), who find for 7 US states that legislators bring less "pork" to their district when they cannot seek re-election anymore –i.e. once being faced with a term limit-, but that this reduction is smaller in states with many electoral districts and hence larger legislatives. However, Pettersson-Lidbom (2012a) criticises the Aidt and Shvets study for not clustering standard errors at the electoral district level[296], finding that the latter would increase standard errors by 50%. Pettersson-Lidbom (2012a) also criticises the Aidt and Shvets study for not including electoral district fixed effects, as a consequence of which their identification is largely based on *between* district variation. The latter may of course suffer from correlation with unobservables[297].

Egger and Köthenbürger (2010) bring a considerable methodological improvement to the Law of 1/n literature, as they are the first to make use of an RDD to demonstrate a positive effect of council size on government spending for municipalities in the German state of Bavaria. In particular, this study exploits discontinuities in the legal rule that relates population size of a municipality to council size[298] to identify a

---

by a norm of "universalism" –or "deference"- as opposed to a norm of "a minimal winning coalition": as all US Congress's members have agenda-setting power, the result is nearly unanimous approval of legislative proposals. ("You scratch my back, I'll scratch yours.") "N" can be argued to be larger in "universalistic" legislatives than in "minimal winning coalition" legislatives. (In the latter, typically only a limited subset of members have agenda-setting powers.) US Congress's "universalism" is the context in which Weingast e.a. (1981) proved their Law of 1/n theoretically.

[295] However, council sizes of US cities may not be endogenous to spending anyway. Baskaran (2013 p. 365) points out that the problem of endogeneity of size and fragmentation of the government ("n") is smaller in studies of the municipal level than in studies of the supra-local level, as the political autonomy of municipalities is evidently generally much smaller than that of the supra-local level. An illustration of the lack of autonomy of the municipal level over its size of government are the discontinuities that Pettersson-Lidbom (2012a) uses for identification in his investigation of the Law of 1/n for Swedish and Finnish municipalities. These are the demographic thresholds that –once surpassed- *legally force* the Finnish and Swedish municipalities in question to increase the size of their municipal council.

[296] A similar critique applies to Baqir (2002).

[297] Additionally Aidt and Shvets (2012) do not make use of an IV approach or RDD to take into account endogeneity of their independent variable of interest.

[298] As in Sweden and Finland, the Bavarian inhabitants-per-municipality thresholds legally force –once surpassed, even if by 1 inhabitant- the municipality in question to increase the size of its municipal council.

causal relationship between council size and public spending. Moreover, the authors show that municipalities primarily adjust current expenditure in response to a rise in council size. However, Ade and Freier (2011) demonstrate the population thresholds used by Egger and Köthenbürger (2010) to be endogenous, as they appear prone to manipulation and as they are thresholds at which a number of other variables apart from council size change.

Using the same methodology and similar thresholds as Egger and Köthenbürger (2010), Pettersson-Lidbom (2012b) is one of the few studies that we are aware of that finds empirical evidence for a "reverse Law of 1/n" in the spirit of Primo and Snyder (2008). In effect, Pettersson-Lidbom (2012b) finds a *negative* causal relationship between council sizes of Swedish and Finnish municipalities –which are uniquely determined by population thresholds- and public expenditures. Pettersson-Lidbom's explanation is that civil servants –by definition not up for re-election- at the fiscally very important municipal levels of Sweden and Finland show more spending restraint if controlled by more local politicians –by definition up for re-election.

4.2. *"Indirect"* empirical investigations of the Law of 1/n

Apart from that it is inspired by the *"direct"* empirical investigations of the Law of 1/n reviewed above, i.e. investigations that give a purely *legislative* meaning to "n", our research is also related to the -generally less recent- investigations of the effect of fragmentation of the *executive* on the *budget balance*, rather than on public expenditures as such. Indeed, it seems to make sense to define the common pool problem either in terms of overall expenditures or in terms of the overall budget balance: individual politicians could favour their constituency either with extra expenditures or with a lower tax burden or with both.

The studies on *government fragmentation* can be considered a rather *"indirect"* test of the Law of 1/n, as "n" interpreted broadly may also relate to the number of interest groups / veto players represented in the *executive*. Another difference with *"direct"* empirical investigations of the Law of 1/n is that *"indirect"* studies are mostly cross-country, as most of them study fragmentation of *central* governments, while most *"direct"* studies investigate fragmentation of *local* governments. A 3rd major difference is that the *"indirect"* studies stand a higher chance of finding a statistically significant effect, as of course fragmentation of the executive –either in terms of number of ministers or in terms of number of coalition parties- is a more time-variant variable than "n" in its strict meaning, i.e. number of electoral districts or number of MPs. E.g. in the case of Belgium, changes in the latter mostly only happened in the wake of or in parallel with a change of the constitution over our sample period[299]. The final, and perhaps major, difference between the "government fragmentation" literature and "direct" empirical investigations is that in the former the use of country fixed effects as well as IV estimation appear even less standard, making its results even more vulnerable to criticisms related to omitted variable bias and reverse causality.

[299] Of course, "n" in the legislative sense and "n" in the executive sense are not wholly independent from each other. If smaller electoral districts mean less political parties represented in parliament, they also mean that the governing coalition stands a higher chance of being composed of less parties. E.g. the Flemish parliament in 2003 reduced its number of electoral districts from 17 to 6, i.a. motivated by the fact that smaller (and hence more) electoral districts put smaller political parties at a disadvantage during elections, also given the D'hondt system of translating vote shares into seat shares. (It was a small party (Spirit) that was the major promoter of the decrease in Flemish districts.)

The negative consequences of fragmentation of the executive on the budget balance have been *theoretically* established by Alesina and Tabellini (1992), Alesina and Perotti (1994), Velasco (1997), and Persson and Tabellini (2000). The same negative consequences have been *empirically* analysed for the first time by Roubini and Sachs (1989). Roubini and Sachs (1989) in particular demonstrate that large, short-lived and uncohesive coalition governments lead to large *budget deficits*[300]. Roubini and Sachs view avoiding deficits within one and the same government as a prisoner's dilemma: all parties may prefer a balanced budget, but in the absence of strong coordination between them to achieve such a cooperative outcome, each party may have an incentive to protect and expand "its" part of the budget. Additionally, while cooperation of *all* coalition partners may be needed to *avoid* a deficit, non-cooperation by *one* of them may suffice to *cause* a deficit. Spolaore (1993) comes to an empirical finding that is similar to the one of Roubini and Sachs (1989): the higher the number of parties in a coalition, the larger the deficit in response to a negative economic shock.

In the same vein, but limiting their sample to US states, Alt and Lowry (1994) find deficits more likely under divided government –i.e. with the governor, the House of Representatives' majority, and the Senate's majority not all 3 belonging to the same party. However, it is unclear why Alt and Lowry split their sample of state – legislature observations in subsamples according to the degree of division of state governments –which in some cases reduces their number of observations to only 59-, rather than to make use of interaction terms within one and the same sample. Not clear either is the extent to which Alt and Lowry include state fixed effects into their regression analysis. Also Volkerink and de Haan (2001) and Perotti and Kontopoulos (2002) make the finding of Roubini and Sachs more specific, by providing empirical evidence that the number of *ministers* cause the budget deficit to increase more strongly and significantly than the number of *parties* in a coalition per se. Perotti and Kontopoulos (2002 p. 213-214) find that the number of ministers and the number of parties are an even stronger determinant of the budget deficit in times of economic crisis ("difficult times")[301].

More recent are a number of *within-country* empirical studies of the Law of 1/n with respect to the executive. Ashworth e.a. (2005) find for Flemish municipalities that governments comprised of more parties, as well as of ideologically more heterogeneous parties, lead to larger debts, hence to larger deficits. Schaltegger and Feld (2009) find for the Swiss cantons that public expenditures increase with the number of ministers –rather than with the number of political parties in the governing coalition- per canton. However, because Schaltegger and Feld's independent variables of interest do not vary much within individual cantons, their identification is largely based on between-variation, rendering their finding vulnerable to omitted variable bias. Schaltegger and Feld do not explicitly tackle the problem of reverse causality either.

Most recently, Baskaran (2013) finds for German states that neither the number of ministers nor the number of parties of which the states' executives are comprised affect total spending, either in terms of statistical or in terms of economic significance. Interestingly, Baskaran's identification of the effect of the

[300] However, de Haan and Sturm (1997) replicated Roubini and Sachs' (1989) research, and failed to find a relationship between government fragmentation and budget deficits.

[301] However, as pointed out by Pettersson-Lidbom (2012a), Perotti and Kontopoulos do not account for serial correlation in their panel data regressions.

number of coalition parties on expenditures is based on an IV approach that is in turn based on the German system of an electoral threshold of 5% of the vote, holding in any state parliament. This threshold creates exogenous variation in the number of parties represented in state parliaments –in that in some cases some parties happen *just* to pass the electoral threshold and that in some other cases some parties happen *just to fail* to pass the electoral threshold, in turn influencing the probability that the largest party either forms a single-party or a coalition government[302]. However, while Baskaran finds an interesting solution for the endogeneity of his political variable of interest "number of parties in the coalition", he assumes that his alternative measurement of fragmentation, i.e. "number of ministers in the government", is exogenous to public spending. In contrast to Baskaran, we will assume that also the number of ministers may be endogenous to public spending and we will develop an IV strategy to solve this problem.

Finally, while Baskaran does not find a Law of 1/n to hold with respect to the executive, 2 studies even find an executive-related "reverse Law of 1/n" to hold. Exploiting a legal discontinuity faced by parties governing Bavarian municipalities, Freier and Odendahl (2012) show in RDD-type estimations that one party governments spend *more* -not less- than coalition governments, in particular with respect to investment spending. Freier and Odendahl find weak evidence that such governments also increase the deficit. They use a new method to detect close election outcomes in multi-party systems, as a result of which they are able to isolate exogenous variation in the type of government. Another "reverse Law of 1/n" study with respect to the executive is the one by Gagliarducci and Nannicini (2013), who show that *better-paid* mayors reduce spending. I.e. higher wages attract more competent and educated candidates into politics and once elected, those candidates "size down the government machinery by improving efficiency". Hence, the meaning of "n" in the case of Gagliarducci and Nannicini (2013) is not the *number* of politicians but their *direct budgetary cost* in terms of their salary. Similarly to Freier and Odendahl (2012), the study on Italy makes use of an RDD, as the wages of Italian mayors depend on population size and sharply rise at different thresholds.

The studies by Ashworth e.a. (2005), Schaltegger and Feld (2009), Freier and Odendahl (2012), Gagliarducci and Nannicini (2013), and Baskaran (2013), as well as the much older one by Alt and Lowry (1994), stand out from the empirical government fragmentation literature because they focus on one single country, thereby running a lower risk of running into the problem of unobserved time-variant heterogeneity that typically plagues cross-country studies of the fiscal effects of government fragmentation. Additionally, the studies by Ashworth e.a. (2005), Freier and Odendahl (2012), Gagliarducci and Nannicini (2013), and Baskaran (2013) explicitly acknowledge the possible endogeneity of their independent variable of interest even within the context of one and the same country, and adapt their empirical strategy to this problem.

Table 3 summarises the empirical literature on the Law of 1/n that we overviewed above, according to the major criteria on the basis of which we can distinguish between the respective contributions to this

[302] For the case of Belgium, this IV approach cannot be used. Firstly, in Belgium an electoral threshold holds only since the 2003 Federal elections and since the 2004 Regional elections. Secondly, in Belgium a party just passing or just *not* passing the electoral threshold does not affect the probability of a single party government being possible. Over our sample period, no single party came even close to obtaining 50% of the seats in any supra-local parliament and over our sample period often even 3 parties were needed to form a government supported by a parliamentary majority, in contrast to Germany.

literature: definition of "n" (the independent variable), the dependent variable, the approach to endogeneity of "n", the level of government studied, and the finding.

Table 3: Summary of the empirical literature on the Law of 1/n

Definition of "n"	Dependent variable	Endogeneity approach	Level of government	Finding
Size of legislative (# of electoral districts, # of MPs, ...)	Expenditures	None	Central level (multi-country)	No effect
Size of executive (# of parties, # of ministers, …)	Budget deficit	Fixed effects	Supra-local level (within country)	Positive effect
		IV	Local level (within country)	Negative effect
		RDD		

5. Data

We obtained data on expenditures per individual supra-local government –our dependent variable-, as well as government revenue data –split up between grants and own taxes raised- from 1989 onwards from the "Conjunctuurnota". The "Conjunctuurnota" is a publication of the Federal Ministry of Finance (which in Belgium is a separate ministry from the Federal Ministry of the Budget). Remarkably, we have not found expenditure and revenue data per supra-local government for earlier years[303]. The exception is the Federal government, for which the "Conjunctuurnota" allows us to collect expenditure data from 1982 onwards. Another exception is VGC, for which we have not found expenditure data in the "Conjunctuurnota". We had to make use of the Ministry of the Budget's "Algemene Gegevensbank FOD B&B"[304], although this source only shows expenditures for VGC from 2002 onwards.

Graph 1 shows the evolution of our dependent variable, i.e. annual expenditures *per capita* per individual government considered over our sample period (in real euros of 2012) (as opposed to average *total* expenditures per government over time, shown in table 2). The expenditure variable we consider is net of interest expenditures for all governments, because we assume that the latter are not under the control of the government, and moreover cannot be directed to a politician's electoral district or constituency[305]. The

[303] The Federal Ministry of the Budget, to which the Federal Ministry of Finance referred us to for more detailed data for more years, replied to us that the earliest data on supra-local expenditures and revenues it had available concern the year 1997. We have not been able either to obtain expenditure data pre-1989 from the National Audit Office ("Rekenhof"), which is in charge of budget execution reporting / ex post auditing with respect to all supra-local Belgian governments. Various sources that we consulted at the level of the individual governments of the Regions and Communities themselves ultimately referred us to the individual annual budget laws of the 1980s. We have not found the time yet to consult individual budget law per budget law.

[304] http://www.budgetFederal.be/NL/figures/Pages/databank.aspx

[305] Evidently, the expenditures of the Federal government we consider are also net of grants to the other supra-local governments, all of which were mainly grants-financed over our sample period. Not only do we avoid to "count

considerable between variation with respect to expenditures was already clear from table 2 above. Here we briefly discuss within variation.

Most striking is once again the evolution of Federal expenditures. Their *gradual* decrease in the 1980s is a reflection of the expenditure reducing efforts by the "turnaround" government that came to power end of 1981, and that was reelected in 1985. These efforts were in turn motivated by the snowballing interest expenditures of the 1980s, which in turn stemmed from the very large deficits incurred during the end of the 1970s and the beginning of the 1980s. In contrast, the *steep* decline in Federal spending between 1988 and 1989 is due to the 3rd "state reform". This was the largest "state reform" in Belgian history fiscally speaking, in that important expenditure responsibilities such as education were devolved to the Communities and other important expenditure responsibilities such as industrial policy and infrastructure were devolved to the Regions. Another Federal expenditure shock happened in 2005, as the then Federal government had to revise its original accounting for the 2005 expenditures considerably upwardly after the European Commission obliged the Federal government to include its takeover of debt from the public railways in 2005 into the budget. In contrast, expenditures of the other 8 supra-local governments evolved more smoothly, though most of them steadily increasing over time. The general picture is therefore one of overall primary public expenditure in Belgium steadily increasing over time[306]. Graph 1 also shows that our panel is unbalanced not only because the Federal government is the only one for which we obtained expenditure data concerning the 1980s, but also because the first years for which we obtained expenditure data concerning COCOF, VGC and GGC are even more recent than 1989, the year in which they came into existence[307].

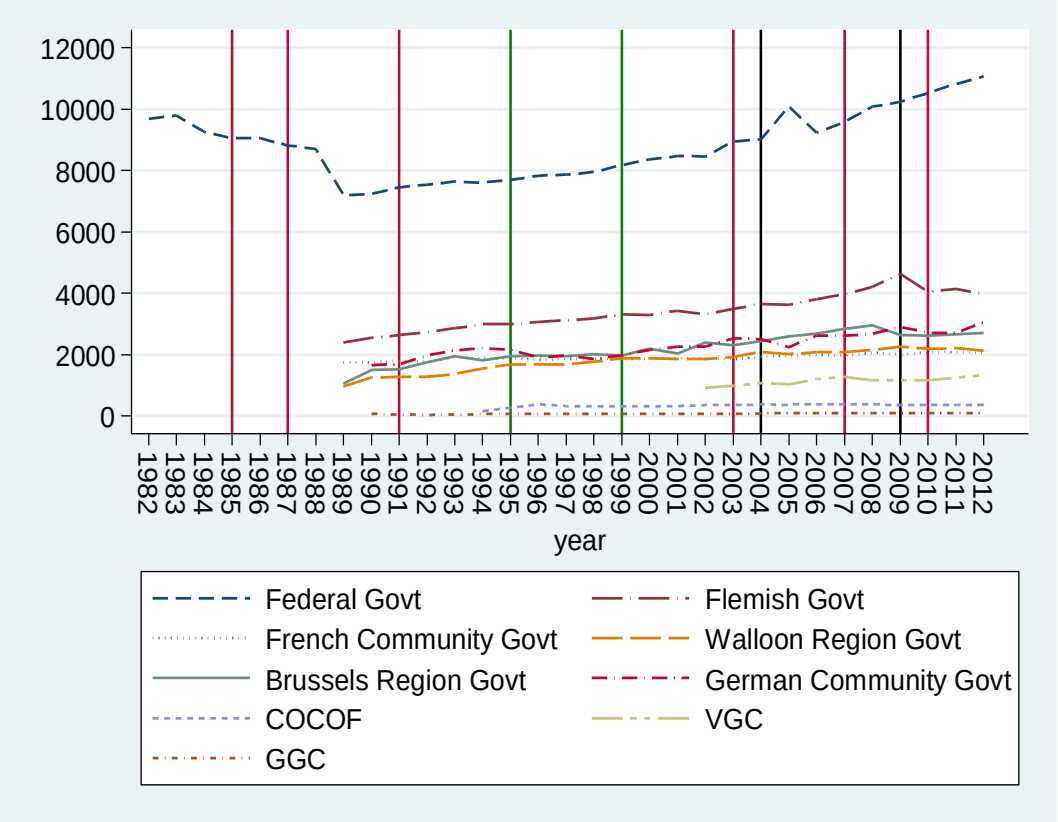
---

some expenditures twice" in so doing. But also we hence consider the size of those grants as independent from the size ("n") of the Federal government. Indeed, those Federal grants are formula-based, and can only be changed by means of a "state reform". Of course the executive may consist of many parties *precisely with a view to* implementing a state reform. This is because "state reforms" need approval of 2/3 of Federal MPs, as well as of both a (simple) majority of Dutch-speaking and French-speaking MPs. We will control for "state reforms" in our regression analysis below.

[306] This picture remains when expressing expenditures as a share of GDP, so that the increase shown in graph 1 is not just due to economic growth. As said above, taking the first and the most recent year available in OECD statistics, total public expenditure in Belgium increased from 51.1% in 1997 to 54.4% of GDP in 2013.

[307] Most striking is the VGC, for which we only obtained expenditure data from 2002 onwards. The VGC invited us to their paper archives to collect expenditure data for years preceding 2002, but we have not yet found the time to go and visit these archives.

Graph 1: (Primary) expenditures per capita per government (1982-2012) (in real euros of 2012)

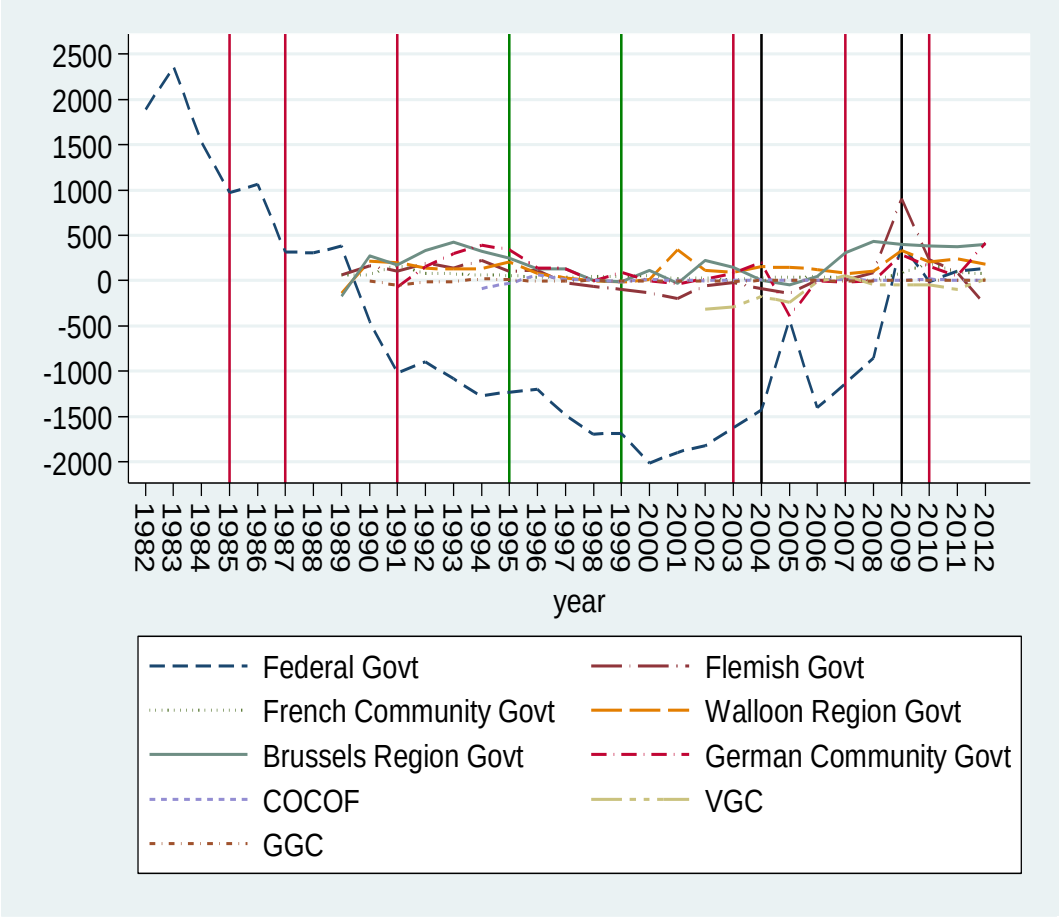


Note: red vertical bars indicate Federal election years; green vertical bars indicate concurrent Federal and Regional election years; black vertical bars indicate non-concurrent Regional election years

Sources: "Conjunctuurnota" of the Ministry of Finance; website of the "Algemene Gegevensbank FOD B&B" (http://www.budgetFederal.be/NL/figures/Pages/databank.aspx)

Because we will not only investigate the Law of 1/n for Belgium with respect to (primary) expenditures but also with respect to the (primary) deficit, graph 2 shows the (primary) budget deficit (+ for deficit; - for surplus) per capita per individual government considered over our sample period (in real euros of 2012).

Graph 2: (Primary) budget deficit per capita per government (1982-2012) (in real euros of 2012)



Note: red vertical bars indicate Federal election years; green vertical bars indicate concurrent Federal and Regional election years; black vertical bars indicate non-concurrent Regional election years

Sources: "Conjunctuurnota" of the Ministry of Finance; website of the "Algemene Gegevensbank FOD B&B" (http://www.budgetFederal.be/NL/figures/Pages/databank.aspx)

Most striking from graph 2 is once again the evolution of the Federal (primary) deficit. Also, exception made for the Federal government, between variation with respect to the deficit is much smaller than between variation with respect to expenditures[308]. This is because traditionally an implicit fiscal pact between the supra-local governments stipulates that Regions and Communities would run a balanced budget, and because the Federal government prevented looming Regional and/or Community budget deficits in the past with –implicit, partial, and ex ante- bailouts (i.e. on the occasion of the 1993, 2001, and 2013 "state reforms" (see Jennes 2014b for more detail)). These bailouts keep down variation in Belgian budget deficits. Together with the grants-based rather than tax-based financing of 8 of the 9 supra-local governments, these bailouts seem to make the deficit variable somewhat less promising with a view to explaining it with the number of politicians and their fragmentation.

[308] Also for this reason, the scaling of the Y axis differs between graphs 1 and 2.

Data on a number of demographic and economic control variables per geographic part of Belgium in which a particular supra-local government is active were provided by Statistics Belgium: the number of under 18 years' old, the number of over 59 years' old, total population, and gross private income per capita. Again, remarkably, the demographic control data appear only to exist from 1989 onwards, except for total population covered per individual government and except for the Federal government. Unemployment data were provided by the Federal Ministry of Employment for years from 1982 onwards, and by the Employment Agency of the German Speaking Community of Belgium for the German Speaking Community for years from 1990 onwards. As Pettersson-Lidbom (2006 p. 13) writes, such control data are often used in the public finance, fiscal federalism and political economics literature, to capture either costs and benefits of government spending (under 18, over 59), economies of scale in public service provision (total population), fiscal capacity (gross private income per capita), or economic shocks (unemployment) (all assumed to be time-varying).

We obtained the raw data on our political variables of interest from the websites of the individual Belgian parliaments, from the website of the Federal Ministry of the Interior[309], from Cumuleo[310] -a non-governmental website collecting i.a. data on the number of "second jobs" of supra-local politicians in Belgium and their remuneration-, and from the website of the "Algemene Gegevensbank FOD B&B"[311]. We used them to construct 11 political variables of interest, each measuring a different meaning of "n":

- number of ministers per government,
- number of "cabinet members" per government,
- number of political parties making up the governing coalition per government,
- the Herfindahl index[312] of this coalition (after weighting each party according to its share of the parliamentary seats),
- number of ministers' "second jobs" per government,
- number of MPs per parliament,
- number of MPs supporting the government per parliament,
- number of electoral districts per parliament,
- number of political parties that obtained parliamentary representation per parliament[313],
- the Herfindahl index of this parliamentary composition (after weighting each party according to its share of the parliamentary seats),
- and the direct cost of politicians per government.

"Cabinet members" differ from ministers in that they also comprise so called "secretaries of state" and "government commissioners", i.e. junior ministers who only characterise the Federal and the Brussels Regional governments, and who are distributed over coalition parties essentially to better translate election

[309] http://www.ibzdgip.fgov.be/result/nl/search.php?type=year
[310] http://www.cumuleo.be
[311] http://www.budgetFederal.be/NL/figures/Pages/databank.aspx
[312] As is well-known, a Herfindahl index of 0 shows maximum fragmentation of power, while a Herfindahl index of 1 shows maximum concentration of power.
[313] Although for Germany, Baskaran (2013 p. 357) does not a priori see any direct effect of the number of parties in parliament on spending.

results into the distribution of cabinet posts (and also to circumvent the constitutional rule that Federal ministers should be distributed evenly over Flemish and Francophone coalition parties)[314].

During election years, we use as the value for the political variable concerned the one characterising the outgoing –rather than the incoming government-, as elections never took place earlier in the calendar year than May over our sample period, and as negotiations preceding the establishment of a new government regularly took several months over our sample period.

Table 4a and 4b provide data –when available- on the evolution of the number of "n", i.e. on the number of politicians and their direct cost but also on the number of electoral districts in Belgium between 1982 and 2012. 1982 is the first year of our sample and 2012 is the most recent year for which we were able to collect data with respect to all meanings of "n", i.e. including the direct costs of politicians. Additionally, we show numbers –if available- for 1990 and 1996, as these are the relevant years right after *the implementation of the "state reforms"* of 1988-1989 and 1993[315], which obviously had considerable consequences for "n" in Belgium[316]. Table 4a provides numbers in absolute terms and table 4b provides numbers in relative terms.

The number of ministers was particularly high around the year 1990, with the Brussels Regional government just created in 1989, and the then Federal government (1988-1991) consisting of several extra ministers in charge of implementing the 1988-89 "state reform", which was the largest decentralisation round so far in Belgian history (at least fiscally speaking). Remarkably, the 1993 state reform, which essentially comprised the direct election of members of the Regional and Community parliaments, did not cause an increase in ministers. A similar evolution emerges when we consider "cabinet members" as a whole.

Additionally, we obtained data on the number of remunerated "second jobs" per minister, thanks to the Cumuleo website, which in turn is based on data published by the Belgian National Audit Office ("Rekenhof"). All Belgian "cabinet members" as well as MPs have to report their –remunerated as well as non-remunerated- "second jobs" to the National Audit Office, which may be either private or public sector employments[317]. Our hypothesis behind considering the number of ministers' "second jobs" as another meaning of "n" is that public expenditure and/or the budget deficit would increase with the number of ministers' "second jobs", as each "second job" may mean an extra "constituency" for a minister. We choose to focus on ministers' "second jobs", rather than on "cabinet members"' or MPs' "second jobs", because –as said above- in Belgium ministers are considered more powerful than non-ministerial "cabinet members" and more powerful than MPs. We choose to focus on remunerated "second jobs" rather than on non-remunerated "second jobs", because we hypothesise that the former provide more important

[314] We use the term "cabinet members" for senior and junior ministers taken together, but we put it between quotation marks as we are not 100% satisfied with this term "cabinet members" as a way to distinguish ministers and junior ministers *taken together* from (senior) ministers *considered separately* from junior ministers. This is because junior ministers precisely are *not* allowed to take part in the weekly meeting of ministers (called the "council of ministers" in Belgium, but called the "cabinet meeting" in other countries).

[315] The 1993 "state reform" only generated consequences for "n" in 1996, i.e. after the elections of 1995.

[316] In contrast, the 2001 "state reform" did not have such considerable consequences, except increasing the number of MPs for the Brussels parliaments.

[317] Most frequently, Belgian supra-local politicians have a "second job" as a councillor, alderman, or mayor in their municipality of residence. But many supra-local politicians have more than one "second job".

"constituencies" for a minister than the latter. Unfortunately, Cumuleo only contains data for years from 2004 onwards[318]. Because it is easier to think of "second jobs" in terms of "second jobs" *per minister*, ministers' "second jobs" is the only variable in table 4a that we express in a relative way, i.e. per minister. In 2012, a Belgian minister held on average 1.9 "second jobs". (This number increases to 4.8, if we also consider non-remunerated "second jobs".)

In contrast to the total number of ministers and "cabinet members", the total number of MPs *does* show a continued increase of our sample period. It increases in 1990 due to the establishment of the parliament of the Brussels Region in 1989, and it further increases in 1996, right after the first direct election of the other Regional and Community parliaments in Belgium in 1995[319]. Before 1995, these other Regional and Community parliaments simply consisted of the Walloon, Francophone, resp. Flemish MPs of the *Federal* parliament, a phenomenon that was known as the "double mandate", and which hence effectively kept the number of MPs down until the Regional and Community elections of 1995. (Note that there was no such overlap between the resp. *executives* of the Federal and the Regional and Community governments over our sample period.) The number of Belgian MPs also increased after the 2001 state reform, which comprised an increase in the number of MPs of the Brussels Regional parliament, in an attempt to keep guaranteeing parliamentary representation of the –over our sample period continuously diminishing- Dutch-speaking inhabitants of the Brussels Region.

As for the number of electoral districts, this number evidently increased with the increase in the directly elected Regional and Community MPs in 1995, as the absence of such direct election for a number of supra-local governments before 1995 causes us to consider the number of electoral districts for these governments pre-1995 to be zero. However, the increase in number of electoral districts shown in table 4a between 1990 and 1996 remained rather limited, and even turned into a decrease after 1996, as:

- the 1993 "state reform" also included a –complementary and rather large- reduction of the number of *Federal* electoral districts, both for elections of the House of Representatives and for elections of the Senate,
- in 2002 the Federal government once again decided to considerably reduce the number of electoral districts of the House of Representatives,
- and in 2003 also the Flemish government reduced its number of electoral districts

As should be clear from the last column of table 4a, we unfortunately did not succeed in collecting a large enough panel dataset with respect to the direct financial costs of politicians to include this variable into our regression analysis below[320]. We define direct financial costs of politicians as comprising expenditures made for the financing of Belgian parliaments, for the financing of Belgian "cabinets" of private advisors

[318] As 2004 was the 1st year in which a law came into effect that had already been voted in 1995 and that obliges supra-local politicians to report their "second jobs" to the National Audit Office.
[319] Exception made for the parliament of the German Speaking Community, which has been directly elected over our entire sample period, more in particular ever since its creation in 1974.
[320] As opposed to e.g. Pettersson-Lidbom (2006 p. 11) for his panel regression of governing parties' vote share on specific expenditure categories for Swedish municipalities.

of ministers[321], and for the financing of Belgian political parties. By means of illustration, we show that in 2012, those direct financial costs amounted to 738 million euros[322]. We did not succeed either in making a breakdown between these 3 components of "direct cost of politicians" per individual Belgian government[323].

Table 4a: Number of "n" in Belgium over our sample period

Year	Ministers	"Cabinet members"	"Second jobs" per minister	MPs	Electoral districts	Direct cost of politicians*
1982	33	43		343	52	
1990	48	64		418	53	
1996	43	48		593	63	
2012	46	55	2	607	49	738

* in million euros of 2012

As said above, table 4b expresses the numbers of table 4a in per capita terms. Unsurprisingly, the evolution of the different meanings of "n" is similar to the evolution shown in table 4a. Of course, the averages in table 4b are not able to show the existing considerable geographic disparity in Belgium with respect to number of politicians and electoral districts: e.g. the number of politicians per capita in the Brussels Region is much higher than the number of politicians per capita in the Flemish Region. This is because no less than 7 of the 9 Belgian supra-local governments are active on the territory of the Brussels

[321] These "cabinets" of private advisors are not to be confused with the cabinet of ministers –called the "council of ministers" in Belgium- as such. Compared internationally, these "cabinets" of private advisors seem large. Their total cost is estimated to be larger than the total cost of parliaments. They are sometimes dubbed "parallel civil service".

[322] Calculated from "Algemene Gegevensbank FOD B&B"; http://www.budgetFederal.be/NL/figures/Pages/databank.aspx

[323] In particular data about the public financing of political parties appear hard to find (see e.g. Smulders e.a. 2014). This is also because Belgian political parties are partly financed indirectly, i.e. via the financing of the Belgian parliaments.

The amount of 738 million euros includes pension payments to former politicians. Over our sample period, Belgian MPs were entitled to a full parliamentary pension after a total of 20 years' membership of a Belgian parliament and from the age of 55. However, not included in the amount of 738 million euros is the extra income many supra-local politicians in Belgium receive thanks to their "second jobs" in the Belgian (local or supra-local) public sector, for lack of data. (Information on the number of individual politicians' "second jobs" is however available at http://www.cumuleo.be/.) Nor does it include the salaries of the "political appointees" in the civil services and state-owned enterprises overseen by the Belgian supra-local governments, equally for lack of data. Informally though not formally, several senior postings in ministries and state-owned enterprises –extending to the central bank, the judiciary, embassies, as well as watchdogs in the field of fiscal policy, competition, and banking- are divided up among the parties that are part of the several coalitions governing the Belgian supra-local governments. These political appointments are used to reward –temporary- advisors in ministerial "cabinets" with a –permanent- position within the government bureaucracy, as well as to maintain the political party in question's influence over the public sector. The press has even reported on senior public sector positions having been newly created to enable ministers to reward private advisors. The system of political appointments is a well-known manifestation of the Belgian "party-cracy".

Region[324] (the exceptions are the Walloon Region and the German Speaking Community governments) but only 2 of them are active on the territory of the Flemish Region only (the Federal government and the Flemish government).

Table 4b: Number of "n" in Belgium over our sample period, expressed in per capita terms

Year	Population per minister	Population per "cabinet member"	Population per minister's "second job"	Population per MP	Population per electoral district	Direct cost of politicians per capita*	Population of Belgium
1982	298,444	229,038		28,713	189,397		9,848,647
1990	208,062	156,046		23,892	188,433		9,986,975
1996	236,517	211,880		17,150	161,432		10,170,226
2012	239,912	200,654	126,076	18,181	225,223	67	11,035,948

* in euros of 2012

Sources for tables 4a and b: websites of the parliaments of Belgium; website of the Federal Ministry of the Interior (http://www.ibzdgip.fgov.be/result/nl/search.php?type=year); Algemene Gegevensbank FOD B&B (http://www.budgetFederal.be/NL/figures/Pages/databank.aspx); http://www.cumuleo.be; Statistics Belgium

Table 5 provides the summary statistics for our political variables of interest. The difference with table 4 is that while table 4 shows *totals for the 9 governments taken together*[325], table 5 shows *averages and standard deviations per individual government*. It should be noted that we have much less observations available for the "ministers' second jobs" and "direct cost of politicians" variables. Together with the fact that the amounts on the website of the "Algemene Gegevensbank FOD B&B"[326] with respect to "direct costs of politicians" for some governments show extreme variation over time, this scarcity of observations makes us exclude the "direct cost of politicians" variable from our regression analysis below.

[324] Including the politicians active at the level of the 19 Brussels municipalities, this has led some to label the Brussels Region as the area with the most politicians per capita in the world.
[325] Except for "second jobs", for which we wish to show the number of "second jobs" *per minister*.
[326] http://www.budgetFederal.be/NL/figures/Pages/databank.aspx

Table 5: Summary statistics for our political variables of interest

Variable		Mean	Std. Dev.	Min	Max	Observations	
Political variables with respect to the executive							
Ministers	overall	6.92	4.10	2.00	19.00	N =	201
	between		4.04	2.00	15.19	n =	9
	within		0.96	4.73	10.73		
"Cabinet members"	overall	8.83	6.14	3.00	32.00	N =	201
	between		5.68	3.00	22.00	n =	9
	within		2.00	2.83	18.83		
Executive's Herfindahl	overall	0.39	0.12	0.16	0.62	N =	201
	between		0.09	0.24	0.53	n =	9
	within		0.08	0.26	0.59		
Parties in the coalition	overall	3.54	1.42	2.00	7.00	N =	201
	between		1.31	2.16	5.42	n =	9
	within		0.67	2.12	5.88		
Government friendly MPs	overall	67.49	47.25	5.00	225.00	N =	201
	between		44.51	8.42	157.88	n =	9
	within		18.18	30.42	152.04		
“Second jobs” per minister	overall	2.30	0.61	1	4.75	N =	81
	between		0.27	1.63	2.60	n =	9
	within		0.55	1.46	4.77		
Political variables with respect to the legislative							
Electoral districts	overall	11.33	13.53	1.00	51.00	N =	201
	between		11.28	1.00	32.44	n =	9
	within		7.55	-7.11	29.89		
MPs	overall	107.60	76.26	12.00	318.00	N =	201
	between		73.37	15.75	263.44	n =	9
	within		22.60	65.16	162.16		
Legislative's Herfindahl	overall	0.22	0.06	0.11	0.35	N =	201
	between		0.05	0.13	0.30	n =	9
	within		0.02	0.15	0.27		
Parties in parliament	overall	7.50	2.88	4.00	13.00	N =	201
	between		2.85	4.75	11.53	n =	9
	within		0.70	5.97	8.97		
Political variables with respect to the executive + legislative							
Direct cost of politicians	overall	96.40	131.00	2.50	443.00	N =	68
(millions of euros of 2012)	between		149.00	3.73	389.00	n =	9
	within		21.60	22.60	160.00		

Sources: websites of the parliaments of Belgium; website of the Federal Ministry of the Interior (http://www.ibzdgip.fgov.be/result/nl/search.php?type=year); website of the “Algemene Gegevensbank FOD B&B” (http://www.budgetFederal.be/NL/figures/Pages/databank.aspx); http://www.cumuleo.be

It is not surprising that for almost all variables shown in table 5, the between standard deviation is (far) larger than the within standard deviation. The large between standard deviations are of course mainly due to the large difference in size between the 9 governments considered (see table 2), but they also could be due to time-invariant or time-variant unobserved factors. The small within standard deviations are of course mainly due to the fact that our political variables are at the same time the manifestation of institutions, institutions that mostly do not vary much over time. The small within standard deviations will pose a challenge to our Fixed Effects estimation strategy below. In particular within variation with respect to number of MPs and number of electoral districts is almost exclusively due to changes following "state reforms" (and hence changes of the constitution).

As an elaboration of table 5, annex 2 *graphically* shows between and within variation for our political variable of interest of which the coefficient will prove rather consistently significant in the regression analysis below, i.e. the number of ministers.

6. Empirical strategy

As said above, an advantage of limiting our test of the Law of 1/n to *Belgian* governments is that we avoid to some extent the problem of omitted unobserved variables that explain between-government heterogeneity in a multi-country study. However, we still need to take into account the possibility of -time-invariant and time-variant- unobservables explaining heterogeneity of our variables of interest between the Belgian supra-local governments considered. It is true that all 9 governments considered are "parliamentary" (as opposed to "presidential") regimes with a proportional (as opposed to majoritarian) electoral system, and that all 9 are the result of the Belgian state that came into existence in 1830, and of the Belgian state's history since then. However, the Francophone dominated governments (Francophone Community, Walloon Region, Brussels Region, and COCOF), the Flemish dominated governments (Flemish government and VGC), as well as the German Speaking Community government may be argued to be affected by idiosyncratic cultural and political factors. This holds true in particular since the 1930s, when Dutch became the only official language in Flanders and when Brussels became officially bi-lingual, and even more since the 1960s, when German became the only official language in the German speaking part of Wallonia. Another argument in favour of the importance of idiosyncratic cultural and political factors is that in the 1970s all major parties "communitised" into separate Francophone and Flemish wings, due to which since then virtually all parties represented in the Belgian parliaments collect votes only in one of the 3 Belgian Communities[327].

Moreover, among the 9 governments considered, the Federal government is "in a league of its own". It remains by far the largest of the 9 supra-local Belgian governments, and it is still being prioritised by Belgian political parties. It is by far the only supra-local Belgian government that is fully tax-autonomous, and traditionally it also incurs the largest budget deficits while in contrast the threat of considerable deficits of the Regional and Community governments has repeatedly been answered by the Federal government with an increase in grants (at the occasion of the 1993, the 2001 and –outside our sample period- the 2013 state reforms). Moreover social security was an exclusively Federal responsibility over

[327] As said above, with the exception of the federal electoral district of Brussels-Halle-Vilvoorde, straddling the Brussels and the Flemish Region, and hence the French Community and the Flemish Community.

our sample period, the entitlement nature of which arguably renders the Federal budget to be the least "flexible" of all governments considered. Because of its full tax autonomy and its monopoly over social security, the Federal government is also the Belgian government that is by far the most exposed to the international economic cycle[328], sometimes to its benefit, sometimes to its detriment. Other institutional reasons for which the Federal government stands out are the facts that it is by far the eldest of the 9 governments –actually the only government pre-dating 1970-, that the Federal parliament is the only bicameral parliament in Belgium, and that –together with the Brussels Regional parliament and government- the Federal parliament and government are the only ones of the 9 parliaments and governments that are organised "confederally", i.e. into Francophone and Dutch language groups, with as a side-effect that the Federal and Brussels government are the only governments with junior ministers. Finally, it is the only of the 9 governments that has seen its expenditure responsibility and tax raising power being *reduced* over our sample period, to the benefit of the other 8 supra-local governments.

For all these heterogeneity reasons, we will implement our regression analysis while including government Fixed Effects (FE)[329]. FE take care of the problem of omitted time-constant drivers of expenditures and/or the budget deficit that may also be correlated with our political variables of interest. But another problem with respect to our research aim may be reverse causality: e.g. a government that has set itself an expenditure increase (reduction) as a goal may precisely consist of many (few) ministers and/or parties –either as a way of signaling or committing itself to its intentions or because expenditure increases (decreases) are more feasible with many (few) ministers and/or parties[330]. (see also Schaltegger and Feld 2009 p. 39 and Pettersson-Lidbom 2012a p. 269) Therefore, our identification is based on an IV regression approach, instrumenting our political variables of interest with our same political variables of interest but only observed in periods of "state reform". A "state reform" is a Belgian decentralisation round for which the constitution requires that a 2/3 majority in the Federal parliament be collected, a majority for which politicians mostly find it convenient to lay the foundations by inviting many parties to the governing coalition, in turn increasing the number of ministers and junior ministers. "State reforms" can therefore be argued to cause exogenous changes in a number of the meanings of "n". Additionally, "state reforms" have led to changes in "n" in its legislative meaning: e.g. the 1993 "state reform" was the "political" side of the coin of the "fiscal" 1988 "state reform": the 1993 reform reduced the number of Federal electoral districts and Federal MPs hand in hand with the creation of "own" electoral districts and MPs of a number of important Regions and Communities, as well as in response to the strongly diminished expenditure responsibilities of the Federal government decided during the 1988 "state reform".

Changes in "n" understood in its various executive *and even* legislative meanings –such as changing the number of electoral districts- are in general –and also in the Belgian case- impossible without the agreement of the *incumbent* government, which is also in charge of the level of public spending and of the size of the budget deficit. This poses a simultaneity problem that was already pointed out by Baqir (2002

[328] Variation in the Federal government's tax revenues due to the international economic cycle appears larger than variation in the other supra-local governments' grants receipts due to the same international economic cycle.

[329] Perhaps for these same heterogeneity reasons, all empirical studies of the Law of 1/n that we are aware of do not include governments belonging to *different* levels into the *same* sample.

[330] As said above, Baskaran (2013 p. 365) points out that the problem of endogeneity of size and fragmentation of the government ("n") is larger in studies of the supra-local level than in studies of the municipal level, as the political autonomy of the supra-local level is considerably larger. E.g. in Belgium the supra-local level is fully autonomous to change the size of its executives as well as of its legislatives, albeit that changes in the size of the legislative mostly have to be approved by a larger majority than a 50%+1 majority.

p. 1324). As an exception to this rule, the changes in "n" caused by a "state reform" are not incumbent-driven but constitutionally driven and hence exogenous to public spending and to the budget deficit. However, one could counter-argue that Belgian "state reforms" *as such* may have been strongly related to expenditure issues, and therefore to budget balance issues:

- Expenditure responsibilities of course *changed* following most Belgian "state reforms".
- There is empirical evidence available that fiscal decentralisation *impacts* overall public spending: upwardly when fiscal decentralisation is grants-based (like in Belgium), and downwardly when fiscal decentralisation is tax autonomy based. (See e.g. Inman (2008) and Eyraud and Lusinyan (2011).)
- A number of Belgian "state reforms" have been examples of logrolling or vote-trading: Flemish parties typically demanded more *competencies* for the Flemish government level, while Francophone parties typically demanded more *funding*. However, this extra *funding* mostly took the form of more Federal grants to the Regions and/or Community governments, while we take care to include revenues (including Federal grants for all governments considered except for the Federal government) as a control variable and while we take care (with respect to the Federal government) to exclude Federal grants to the other supra-local governments from Federal expenditures considered.
- Spending may have particularly gotten out of hand during Belgian "state reform" governments, as "state reforms" may have distracted governments –in particular the Federal government[331]- from conducting a strict fiscal policy. E.g. conventional wisdom in Belgium has it that there is a relationship between the very poor fiscal policy of the –many and short-lived- Federal governments during the 2nd half of the 1970s and the repeated –but failed- attempts of these governments to agree on and implement a "state reform". Also with respect to our sample period, both the Federal government in charge of the 1988-89 "state reform" and the one in charge of the 2001 "state reform" were known for fiscal expansion. However, the 1988-91 fiscal expansion is generally ascribed to the coalition switch that happened: the Christian-democrats exchanged the –at least at the time- more fiscally conservative liberals for the –at least at the time- more fiscally expansionist socialists. Similarly, the 1999-2003 fiscal expansion is generally ascribed not to the 2001 "state reform" but to Belgium having secured membership of the Eurozone in 1998-99.
- Conversely, Belgian public spending as well as the budget balance having gotten out of control since the 1970s may well have been one of the *causes* of demands for expenditure decentralisation (instead of having been one of its consequences), in particular on behalf of Flemish politicians.

For all these reasons that "state reforms" could be *directly* related to spending and the budget balance, and not only by means of their relation to "n", we will attempt to control for this *direct* effect of "state reforms" both in our FE and in our IV regression analysis (see below). I.e. we will include dummies that each attempt to capture the effect of one of the 3 "state reforms" over our sample period, i.e. taking the value of 1 starting with the 1st year in which each "state reform" took effect, i.e. resp. from 1989, 1996, and 2002 onwards.

Also, an IV regression strategy is hoped to take care of the problem of multicollinearity between our several political variables of interest. Indeed, a problem for our regression analysis below is the in some cases strong correlation between our 9 individual political variables of interest, as shown by the

[331] But possibly also the other supra-local governments, as those were governed by coalitions that were "symmetrical" to the Federal government coalition over most of our sample period.

correlation matrix in annex 1[332]. Those strong correlations make that if we regress government spending and/or the budget deficit on several of these political variables at the same time, we risk facing multicollinearity problems. Therefore we will not simultaneously include political variables that are obviously strongly correlated, such as number of parties in the coalition and the executive's Herfindahl index, or number of parties in parliament and the legislative's Herfindahl index. Additionally, our strategy will be to reduce multicollinearity by instrumenting individual political variables of interest.

Our regression equation estimated in the regression tables below may hence be summarised as follows:

$$public\ expenditures\ /\ budget\ deficit\ _{it}$$

$$= \alpha_1\ "n"_{it} + \alpha_2 X_{it} + \alpha_t + \alpha_i + \varepsilon_{it}$$

With "n" our political variable of interest, with X_{it} a set of financial, demographic, economic, and political (i.e. state reform) control variables, with α_t a year fixed effect, and with α_i a government fixed effect.

Regression results for our empirical strategy are shown in a stepwise way in the tables below.

7. Results of the regression analysis

7.1. Fixed Effects (FE) regression analysis

Table 6 presents the results of the simple FE regression analysis. Note that we have 201 observations at our availability (1 government * 31 years; 4 governments * 24 years; 1 government * 23 years; 1 government * 19 years; and 1 government * 11 years). In line with the recent empirical literature on the Law of 1/n (e.g. Schaltegger and Feld (2009), Brooks e.a. (2011), Aidt and Shvets (2012), Pettersson-Lidbom (2012b), and Baskaran (2013)), we opt to express our political variables of interest in absolute terms, and our dependent variable as well as our financial, economic and demographic control variables in relative terms, i.e. divided by population, to take into account the varying weights of the 9 governments relative to the population they "cater to". As announced above, our strategy is to add government fixed effects to all regression specifications, so as to control for relevant time-constant unobservables.

Column (1) shows results for a regression only including our number of ministers variable and (not shown) a set of year dummies, showing the unexpected sign for the minister coefficient, which is moreover significant at the 0.1 level. We start with only including the ministers variable as our variable of interest, as arguably ministers (and their "cabinets" of private advisors) are the most powerful actors of the executive and the legislative considered together (see e.g. Timmermans (1994) and Dewachter (2001)).

[332] Obviously some correlations between our variables measuring "n" are (strongly) *negative*, as both Herfindahl indices *decrease* with "n" (be it parties in the coalition or parties in the parliament) increasing.

Table 6: FE regression of (primary) expenditures / deficit per capita on political variables measuring "n"

	(1) spending pc	(2) spending pc	(3) spending pc	(4) spending pc	(5) **deficit pc**	(6) spending pc	(7) **deficit pc**	(8) spending pc	(9) spending pc	(10) spending pc	(11) spending pc	(12) spending pc	(13) spending pc
ministers	**-173.3***	**-61.74**	**-34.19**	**44.44***	**55.46***							**30.67**	**39.02**
	(89.24)	**(49.03)**	**(32.83)**	**(19.73)**	**(25.09)**							**(44.16)**	**(35.52)**
"cabinet members"						**27.31*****	**17.65*****						
						(3.972)	**(2.589)**						
parties in government								**-3.277**				**-2.399**	
								(14.35)				**(20.81)**	
executive's Herfindahl									**-137.4**				**-278.2**
									(177.6)				**(304.3)**
govt friendly MPs										**0.875**		**0.304**	**-1.185**
										(1.031)		**(2.671)**	**(2.037)**
ministers' "second jobs"											**-3.225**		
											(5.057)		
electoral districts												**-29.59***	**-33.72**
												(15.63)	**(20.62)**
MPs												**6.095**	**7.241**
												(4.484)	**(5.561)**
parties in parliament												**-2.311**	
												(57.18)	
legislative's Herfindahl													**1804.2**
													(1748.5)
revenues pc		0.651***	0.469	-0.0692		-0.0484		-0.0874	-0.0963	-0.0906	0.0475	-0.0545	-0.0513
		(0.147)	(0.254)	(0.115)		(0.120)		(0.121)	(0.120)	(0.128)	(0.173)	(0.110)	(0.120)
unemployed pc		13.52	84.96	207.8**	16.88	182.5***	-24.61	161.2**	176.9***	161.1**	503.1	248.1***	265.7***
		(93.50)	(79.02)	(62.42)	(170.0)	(48.20)	(163.8)	(52.14)	(49.11)	(49.59)	(370.4)	(67.03)	(55.56)
under18 pc		-5.312	10.61	-48.14	-67.09	-49.87	-71.08	-54.35	-45.48	-54.19	81.33	-28.07	-21.94
		(21.79)	(21.78)	(29.78)	(39.17)	(29.89)	(43.95)	(34.33)	(32.69)	(34.80)	(124.2)	(18.36)	(21.33)
over59 pc		17.21	35.89	162.7	159.4	130.9	98.50	96.32	97.43	90.99	72.45	161.8	114.6
		(32.68)	(42.14)	(105.2)	(160.2)	(96.72)	(139.3)	(109.7)	(104.5)	(109.4)	(52.68)	(134.8)	(138.6)
grossincome pc		0.133	0.119	0.115*	0.105	0.0957	0.0607	0.0614	0.0806	0.0711	-0.0903	0.140**	0.133**
		(0.104)	(0.0757)	(0.0567)	(0.112)	(0.0611)	(0.127)	(0.0741)	(0.0741)	(0.0670)	(0.554)	(0.0484)	(0.0449)
state reform dummies			X	X	X	X	X	X	X	X	X	X	X
govt - year trend				X	X	X	X	X	X	X	X	X	X
N	201	201	201	201	201	201	201	201	201	201	81	201	201

Robust standard errors clustered at the government level in parentheses. Year and government dummies and (from column (4) onwards) government-year trends included, as well as (from column (3) onwards) dummies for the 3 "state reforms" over our sample period, taking effect in resp. 1989, 1996, and 2002, and the interaction of these "state reform" dummies with a Federal government dummy. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column (2) shows the results of a specification additionally including a set of financial, economic and demographic control variables, expressed in per capita terms. Revenues[333] and gross income are expressed in euros per capita, while the number of unemployed, under 18, and over 59 variables are expressed as population shares (%) taking a value between 0 and 100. Hence the different magnitudes of the coefficients on the former variables compared to the latter variables. One could imagine that these controls are both correlated with public spending and with our political variables of interest: e.g. if gross income per capita decreases or if the unemployment rate or the population share of the old increases, demand for public spending may increase *and* a newly elected government may wish to respond to this increased demand not only by effectively increasing spending but also by increasing the number of politicians in charge of such increased spending. In most specifications shown in table 6, the coefficient on the share of the number of under 18 years' old shows an unexpected sign, but this variable could as well account for the size of the population at working age (i.e. the *parents* of the under 18 years' old). This is a part of the population that may be less dependent on public spending than other parts of the population. Also the coefficient on revenues shows the unexpected sign in most specifications, although where negative the revenues coefficient is insignificant and very small[334]. One reason could be that revenue may be correlated with gross income per capita, as both of them may measure to some extent the impact of the economic cycle. It is also surprising that among our control variables, only the share of the unemployed shows a significant (and large) coefficient. Once again, a reason could be that gross income per capita may be correlated with the share of the unemployed, as both of them may measure to some extent the impact of the economic cycle. However, surprising about the large and significant coefficient on the number of unemployed is that, as said above, a priori only the Federal government's spending is expected strongly to respond to the economic cycle, as social security spending –including unemployment benefits- was the exclusive competency of the Federal government over our sample period.

The specifications behind column (3) and following additionally include dummies that attempt to capture the effect of the 3 "state reforms" –i.e. decentralisation rounds-, i.e. 3 dummies taking the value of 1 resp. starting with the 1st year in which each "state reform" took effect, i.e. 1989, 1996, and 2002[335]. Additionally, we include (not shown) the interaction of these dummies with the Federal government dummy, as each of the 3 "state reforms" *increased* the expenditure responsibilities of the other 8 governments -hence *reducing* Federal expenditure responsibilities-, as well as *increasing* the Federal grants meant to finance them -hence *reducing* revenues available for Federal expenditures. Moreover, the possibility that public spending would have particularly gotten out of hand during Belgian "state reform" governments, as "state reforms" may have distracted governments from conducting a strict fiscal policy, seems to have applied in particular to the Federal government. It was always the *Federal* government that was in charge of agreeing on and implementing the "state reform" in case. Hence the fiscal effect of the 3

[333] Surprisingly, most of the empirical literature on the Law of 1/n that we reviewed above does not include government revenue as a control. An exception is Baskaran (2013), who finds a significantly positive association between expenditures and revenues.
Our "revenues" variable consists of (formula-based) grants allocated by the Federal government for all supra-local governments except of course for the Federal government itself. Only from the "state reform" of 2001 onwards the revenues of the Flemish government, the Brussels Regional government, and the Walloon government have consisted of own tax revenue to a non-negligible extent.
[334] The coefficients on our variables of interest are however not sensitive to including or omitting government revenues as a control variable.
[335] While the resp. "state reforms" had been voted into law in resp. 1988, 1993 and 2001.

"state reforms" on the Federal government arguably contrasted for several reasons with the effect on the other 8 governments.

Controlling for "state reforms" is important not only because each "state reform" changed expenditures. It is also important because several "state reforms" changed the size ("n") of several *executives* (temporarily: several executives consisted of more ministers and more parties at the time of the 1988 and 2001 state reforms, so as to more easily achieve a 2/3 majority in parliament in favour of the "state reform" in question) as well as of several *legislatives* (permanently: several legislatives experienced a change in their electoral districts and/or number of MPs on the occasion of the 1993 and 2001 state reform).

Of course the way we define our "state reform" dummies assumes that each "state reform" has a *similar* – i.e. permanent- effect in each year starting from its starting year, compared to the year before the starting year. Such a similar fiscal effect is suggested in graph 1 for e.g. the Federal government after the 1988 "state reform" (which was the largest in Belgian history fiscally speaking): Federal expenditures decreased *considerably* between 1988 and 1989, after which they start increasing again (from a considerably lower starting point), but only *gradually*. However, "state reform" effects may well not be constant. E.g. the implicit bailouts of the Community governments included in the 1993 and 2001 "state reforms" had an increasing effect over time. But we hope that our year dummies -as well as the trend variable that we include from column (4) onwards- control for such possible non-constant effects of state reforms.

As expected, in most specifications from column (3) onwards the coefficients on our 3 "state reform" dummies (not shown) are large and significantly positive, while when interacted with a Federal government dummy, the coefficient turns large and significantly negative (as expected). The exception is the coefficient on the interaction of the 2001 "state reform" dummy with the Federal government dummy, which is small and insignificant in most of our specifications. (The latter is not very surprising as the consequences of the 2001 "state reform" on expenditure responsibilities were minor.) Another exception is the non-interacted 1988 "state reform" dummy, which drops out of all our specifications because of multicollinearity[336].

However, when additionally including a trend for every government –to take into account time-variant unobservables that evolve smoothly over time- as shown by column (4), the coefficient on our ministers variable turns positive and significant (p value = 0.05). It shows that an extra minister is on average associated with an increase in expenditures of 44 euros per capita[337]. As the within standard deviation of the number of ministers over sample period is 0.99, the typical variation in public spending per capita associated with variations in ministers over time also equals about 44 euros, with a p value of 0.05. This seems a rather large amount considering average expenditures per capita shown in graph 1 above. Average expenditures amount to 2,809 euros over our sample period, but to no less than 8,819 euros for the Federal

[336] In columns (12) and (13), all "state reform" dummies as well as most year dummies drop out because of multicollinearity.

[337] When we not only include the contemporaneous ministers variable but also a number of lagged ministers variables, the coefficients on one or more of these lags are significant, showing a delayed association between public spending and ministers. This result suggests that only with a delay an increase in ministers is associated with an increase in spending. A similar result holds when we replace the number of ministers with the number of "cabinet members" as the independent variable (see columns (6) and (7)). Including lags of our ministers variable into our IV regression (see table 7 below) produces the similar result that an increase in the number of ministers only causes the deficit to increase with a delay.

government only. Indeed, whenever we omit the Federal government from our regression analysis in column (4) and following, our coefficient of interest considerably reduces in size.

In column (5) we replace our dependent variable "(primary) spending" with the (primary) budget deficit, as the Law of 1/n can be argued –as done above- to hold even more with respect to the deficit than with respect to spending. Indeed, politicians may *also* have an interest in favouring their constituency(ies) with targeted tax reductions, *apart from* targeted spending increases[338]. (Of course in column (5) we no longer control for revenue as our dependent variable itself is the difference between revenue and (primary) spending.) The coefficient on ministers remains of similar size and significance. Surprisingly however, this coefficient represents a very large amount compared to average (primary) deficits per capita –23 euros over our sample period (and 543 euros for the Federal government only)- shown in graph 2 above[339].

When we replace our ministers variable with a "cabinet members" variable, which also includes the number of junior ministers per government apart from the number of senior ministers, as in column (6), our coefficient of interest becomes very significant (p value = 0.00) but smaller in size[340]. However, as the within standard deviation of the number of "cabinet members" over sample period is 1.92, the typical variation in public spending per capita associated with variations in "cabinet members" over time equals about 52 euros (1.92 times 27 euros), with a p value of 0.00. The larger association between spending and "cabinet members" than between spending and ministers is surprising as in Belgium junior ministers are not allowed to participate in the weekly "council of ministers" where government-wide decisions are taken, in particular those with a fiscal impact. Additionally, junior ministers are in charge of less important competencies than "full" ministers are (because of which they are "attached" to a particular "full" minister who has to represent them in the "council of ministers"). Finally junior ministers only have half the budget of a "full" minister at their availability to hire private advisors.

When we once again –similarly to what we did in column (5)- replace our dependent variable "(primary) spending" with the (primary) budget deficit, the coefficient on "cabinet members" remains of similar size and significance, as shown in column (7).

Columns (8) to (11) show regression results of specifications where we again replace our ministers variable with 3 other political variables measuring the size of the executive: the number of parties in the government coalition (column (8)), the Herfindahl index measuring fragmentation of the executive (column (9)), the number of MPs supporting the coalition in power (column (10)), and the number of ministers' –remunerated- "second jobs" (column (11)). All 4 coefficients of interest are insignificant and the "number of parties in the coalition" and "number of ministers' second jobs" coefficients even show the

[338] Such favouring is i.a. suggested by the finding of Jennes and Persyn (2015) that Belgian federal ministers favour inhabitants of their electoral districts with more net formula-based fiscal transfers, i.e. with more social allowances and/or with less social security contributions and/or less income tax payments.

[339] Another reason why the large coefficient on ministers in column (5) is surprising is that –as said above- only 4 of the 9 Belgian supra-local governments enjoyed considerable revenue autonomy over our sample period. These are the Federal government, and –since the 2001 "state reform"- the 3 Regional governments.

[340] When we not only include the contemporaneous "cabinet members" variable but also a number of lagged "cabinet members" variables, the coefficients on a number of these lags are significant, showing a delayed association between public spending and "cabinet members". This result suggests that an increase in "cabinet members" is not immediately associated with an increase in spending, similarly to our result for the ministers variable.

unexpected –negative- sign[341]. The latter negative coefficient could be due to "second jobs" possibly distracting a minister from increasing *overall* expenditures or from increasing expenditures (or from implementing targeted tax cuts) *with respect to his competency (competencies)*. It should also be kept in mind that our number of observations for ministers' "second jobs" is limited, and that a few ministers only may explain the bulk of year-to-year variation as for the number of "second jobs"[342]. (In contrast, the sign of the coefficient on the Herfindahl index is as expected, as the lower the Herfindahl index, the higher the executive's fragmentation, and hence the larger expenditures and/or the deficit are expected to be.) All 4 coefficients of interest are also insignificant when we replace –not shown in table 6- the dependent variable "total spending" with the (primary) budget deficit, as we did in columns (5) and (7).

In table 6 we do not show regression results of specifications where we replace our ministers variable *one after one* with any of our political variables measuring the size of the *legislative*. This is for reasons of space and because we hypothesised that any positive effect of the size of the legislative on overall expenditure and/or on the budget deficit will likely be smaller than any positive effect of the size of the executive. Indeed, none of the coefficients on our separate variables measuring the size of the legislative (not shown) is significant.

As is shown by annex 1, many of our political variables of interest are correlated, which is a reason why all coefficients on our successive variables of interest as shown by columns (1) until (11) may be biased. Therefore, in column (12) we show the regression results of a specification in which we include *all* – executive related as well as legislative related- of our political variables of interest[343]. Because both the number of parties in the coalition and the executive's Herfindahl index, and the number of parties represented in parliament and the legislative's Herfindahl index are substitutes, we take care not to include them simultaneously in one and the same specification. Therefore, column (13) shows regression results after replacing both simple "number of parties" variables –used in column (12)- with both "more refined" Herfindahl indices. As a result coefficients on a number of our variables of interest in columns (12) and (13) show the unexpected sign and one of those –the "number of electoral districts" variable- is even significant in column (12). While in columns (12) and (13) we have reduced the risk of omitted variable bias that we run using the specifications behind columns (4) to (11), we in contrast have increased the risk of multicollinearity, so that none of our specifications shown in table 6 is satisfying.

[341] The sign remains negative and insignificant when replacing the number of ministers' remunerated "second jobs" with the number of ministers' *remunerated and non-remunerated* "second jobs", and when replacing it with the number of "cabinet members"' "second jobs".

[342] Effectively, when inspecting the "second jobs" data, typically a few ministers hold a large number of "second jobs", while most ministers hold only a limited number of "second jobs". Entrance (disappearance) of a minister holding a large number of "second jobs" into (from) a particular government hence typically has a large impact on the total number of "second jobs" per government.

[343] Except "number of second jobs" and "direct cost of politicians", as including one or both of these variables would make us lose more than half of our observations.

7.2. Instrumental Variables (IV) regression analysis

The results of an attempt to avoid *at the same time* the problems of omitted variable bias and multicollinearity are shown in table 7. It presents the results of an IV regression analysis, instrumenting one endogenous political variable *with a subset of that same variable deemed exogenous*[344]. In so doing, we additionally attempt to avoid the possible problem of reverse causality, i.e. a government elected on a platform of expenditure increases may precisely be composed of more ministers, "cabinet members", and/or political parties, to be able to more easily implement those expenditure increases[345]. Alternatively, a government that has seen its expenditure responsibilities increase because of a "state reform" may as a consequence increase its number of ministers. The IV we use is the extra number of "n" that characterised "state reform" governments. Because a "state reform" has to be approved by a 2/3 majority in the Federal parliament, governments implementing a "state reform" are typically larger, i.e. relying on more parliamentary seats, for *constitutional* reasons rather than for *expenditure related* reasons, i.e. so as to secure already within the executive itself most of the 2/3 parliamentary majority needed. Therefore "state reform" governments typically consist of more ministers[346] and more parties, and are –as a consequence of consisting of more parties- characterised by a smaller Herfindahl index.

The Federal, Flemish, and French Community governments of the 1988-91 legislature were characterised of more ministers, more parties and a smaller Herfindahl index than their predecessor and/or successor governments[347]. This was precisely because the 1988-91 Federal government prepared and implemented during this legislature the largest "state reform" in Belgian history. Because at that time there were still only Federal elections (except for the Brussels Regional and the German speaking Community parliament), this larger size of the Federal government was reflected in a larger size of the Flemish and the French Community governments, in terms of ministers. I.e. absence of proper Regional elections facilitated the simultaneous establishment of "symmetrical" –i.e. identical- government coalitions at the

[344] Unsurprisingly the F-test of significance of our IV –shown at the bottom of table 7- is favourable to the relevance of all of our "subset IVs".
Because in all specifications reported in table 7 we instrument the endogenous variable of interest in question with only one IV, we cannot implement nor report the Hansen J test of exogeneity of our IV.
Unfortunately, we are not able to instrument the "number of ministers' second jobs" variable, as it is only observed from 2004 onwards, a time period in which only one government –the 2008-10 Federal government- was larger than usual with a view to implementing a "state reform".

[345] E.g. it is conventional wisdom in Belgium that the largest government in terms of number of parties within our sample, i.e. the Federal government that came to power after the 1999 elections, intended to increase public expenditures from the start, as Belgian politicians had suffered from fiscal consolidation already since the 1991-95 legislature, securing Belgian membership of the Eurozone end of 1998 / beginning of 1999.

[346] As shown by graph 3 in annex 2 below, the exceptions are the governments that agreed on and implemented the 1993, resp. the 2011 "state reforms". They were composed of as many ministers as, resp. *even less* ministers than the previous governments. Our IV therefore takes the value of zero for the governments in power during the years 1992-1995 and 2011-2012. Twice both Green parties –Flemish and Francophone Belgian- provided the parliamentary support the government needed to achieve 2/3 of the parliamentary vote, without those Green parties in return being invited into the government.

[347] Graph 3 does not enable us to compare the number of ministers of the 1988-91 Flemish and French Community governments with their predecessor governments, as it only shows the evolution of the number of ministers for the time period for which we have found expenditure and revenue data. However, the number of ministers of the 1988-91 Flemish and French Community governments was well higher than those of their predecessor governments.

Federal and the Regional level. Unsurprisingly, political parties in power prefer the Belgian Federal and Regional and Community government levels to be party-politically aligned.

Similarly, also the negotiations preceding the formation of the 1999-2003 Federal government included a "state reform". This was one of the reasons that also this government was larger –at least in terms of number of parties, which also results in a smaller Herfindahl index- than its predecessor and successor. Similarly to the 1988-91 legislature, the 1999-2003 Flemish, French Community, and Walloon Regional governments shared these characteristics. Indeed, at that time, Federal and Regional elections were still *concurrent*, with as a side-result largely similar election results, facilitating the formation of "symmetrical" governing coalitions at the Federal and the Regional and Community levels. As said, such "symmetrical" governments are preferred by parties in power. The larger size of the 1999-2003 Federal government was not reflected in more ministers at the Federal level *but* it was well reflected in more ministers making up the French Community and Walloon Region governments. Finally, also the 2007-10 Federal government consisted of more parties than their predecessor and/or successor governments. Note that the latter government *failed* to implement a state reform, which however was firmly the intention of this government[348]. Instead, a "state reform" was agreed and implemented only by the 2011-2014 Federal government.

Column (1) of table 7 shows the regression results of instrumenting the number of ministers variable for all 9 governments with the *extra* number of ministers in those governments during legislatures in which a "state reform" is prepared and implemented. As a consequence, our IV only takes a value different from zero during "state reform" legislatures, and even then for only some of the 9 governments. The coefficient on ministers in column (1) amounts to 53 euros per capita but fails to be significant with a p value of 0.14. It compares to a coefficient of 44 euros per capita that is significant with a p value of 0.06 on ministers as a result of the FE regression in table 6. This result is not very surprising, as IV regression analysis generally tends to result in larger coefficients and lower significance.

In column (2) we replace our dependent variable "(primary) spending" with the (primary) budget deficit, as we did for the specifications in table 6 above in which the coefficient on our variable of interest was significant with the expected sign. Now the coefficient on ministers turns very large and very significant, which is surprising because as said average deficits over our sample period are of course much smaller than average expenditures.

Columns (3) and (4) show regression results when replacing the number of ministers with the number of "cabinet members", and analogously instrumenting it with the number of extra "cabinet members" in governments during legislatures in which a "state reform" is prepared. The 1988-91 and the 1999-2003 "state reform" Federal governments both comprised more junior ministers than their predecessors and successors. In contrast to the regression result shown in columns (6) and (7) of table 6, the coefficients on "cabinet members" in columns (3) and (4) of table 7 are insignificant.

[348] This firm intention showed as after 6 months of fruitless government formation negotiations in 2007, the then government-to-be enlarged its majority by inviting the then largest but one Francophone Belgian political party to the coalition, which was not needed for a simple 50%+1 parliamentary majority. The result was that the government was supported by no less than 94 out of 150 MPs in the Federal House of Representatives.

Table 7: IV regression of (primary) expenditures / deficit per capita on political variables measuring "n"

	(1) spending pc	(2) **deficit pc**	(3) spending pc	(4) **deficit pc**	(5) spending pc	(6) spending pc	(7) spending pc	(8) spending pc	(9) spending pc
ministers	**53.21**	**91.87*** **						**70.67**	**84.30***
	(36.40)	**(26.01)**						**(59.50)**	**(49.53)**
"cabinet members"			**2.634**	**-19.28**					
			(4.907)	**(21.42)**					
parties in government					**-6.218**			**6.607**	
					(16.27)			**(19.02)**	
executive's Herfindahl						**-75.28**			**-707.5***
						(305.7)			**(374.4)**
government friendly MPs							**0.169**	**-3.074**	**-5.258****
							(1.301)	**(2.732)**	**(2.095)**
electoral districts								**-1.692**	**-4.251**
								(20.97)	**(63.42)**
MPs								**2.171**	**3.548**
								(6.709)	**(14.94)**
parties in parliament								**-21.46**	
								(48.75)	
legislative's Herfindahl									**2767.9***
									(1553.8)
revenues pc	-0.0652		-0.0853		-0.0852	-0.0928	-0.0893	-0.0591	-0.0581
	(0.0897)		(0.104)		(0.109)	(0.108)	(0.105)	(0.0849)	(0.105)
unemployed pc	215.9***	57.57	166.6***	-65.38	160.9***	170.7***	164.5***	242.8***	276.8***
	(64.08)	(102.0)	(44.07)	(138.6)	(49.51)	(63.20)	(41.83)	(56.79)	(60.22)
under18 pc	-46.37*	-64.71	-55.06**	-76.22	-57.85*	-49.56	-55.02**	-40.95***	-25.74*
	(24.23)	(39.89)	(27.03)	(47.46)	(30.72)	(35.38)	(27.52)	(15.77)	(14.20)
over59 pc	172.7	220.3**	106.7	47.57	104.8	98.40	101.2	184.9	126.0
	(105.4)	(90.89)	(86.95)	(132.2)	(99.33)	(90.30)	(86.77)	(123.5)	(121.6)
grossincome pc	0.123**	0.151	0.0709	0.0158	0.0656	0.0740	0.0681	0.124**	0.143**
	(0.0521)	(0.0967)	(0.0589)	(0.137)	(0.0626)	(0.0551)	(0.0544)	(0.0531)	(0.0661)
state reform dummies	X	X	X	X	X	X	X	X	X
govt – year trend	X	X	X	X	X	X	X	X	X
N	201	201	201	201	201	201	201	201	201
F-test of joint significance of IVs in 1st stage (p value)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Robust standard errors clustered at the government level in parentheses. Year dummies, government dummies, and government-year trends included, as well as dummies for the 3 "state reforms" over our sample period, taking effect in resp. 1989, 1996, and 2002, and the interaction of these "state reform" dummies with a Federal government dummy. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns (5) to (7) consecutively repeat the IV regression analysis for 3 of our remaining variables of interest related to the executive – the number of parties in the governing coalition, the executive's Herfindahl, and the number of government friendly MPs-, as also for these 3 variables we argue to have a valid and relevant IV available: in each case it is the change induced to them by "state reform" governments or by the "state reforms" themselves. None of the coefficients on the instrumented variables of interest is significant, and 1 of them even has the unexpected sign. Finally, and analogously to the last 2 columns of table 6, we simultaneously include *all* our political variables of interest[349] –while taking care not to include the 2 Herfindahl indices simultaneously with their less refined measures of fragmentation. As a result of this exercise, the coefficients on a number of our variables of interest turn out to be significant, but some of them show the unexpected sign. However, similarly to our discussion of table 6, we should note that several of our IVs are strongly correlated (similarly to the many strong correlations between the original endogenous variables shown in table 6).

Hence, the results in columns (1) and (2) of table 7 are the closest we get to a statistically significant causal relationship between the number of politicians –in our case ministers- and public finance outcomes. We have tested the sensitivity of these 2 IV results to a large number of changes to our regression specification:

- The result in column (1) is robust against omitting the revenues variable from the regression equation. Note that the revenues variable is possibly endogenous to expenditures. E.g. the French Community and the Brussels Region governments have succeeded a number of times since 1989 in obtaining additional grants by increasing their deficits.
- The coefficient on ministers in column (1) becomes significant (52 euros per capita with a p value of 0.09) and the coefficient on ministers in column (2) remains significant (89 euros per capita with a p value of 0.01) after filtering social security –both spending and revenues- out of Federal spending and revenue data[350]. The motivation for filtering out social security is that in Belgium, as in several other countries, the fiscal power of the Federal government over social security revenues and expenditures is limited[351]. This is because social security is to an important extent managed by labour unions and employers' organisations, and because the bulk of social security spending constitutes "permanent legislation" as Marcel (2012 p. 25-26) calls it, almost similar to interest payments on the Federal debt[352].
- When we do not include a trend variable per government in columns (1) and (2), which is a demanding addition given our limited number of observations, our p values improve to 0.02 resp. 0.00 and our coefficients of interest increase to 87 euros per capita resp. decrease to 49 euros per capita.

[349] Due to lack of IVs, we do not instrument the 2 variables measuring parliamentary fragmentation, i.e. the number of parties represented in parliament nor the legislative's Herfindahl index.

[350] Over our sample period, social security was the exclusive competency of the Federal government, and it constituted the bulk of its primary spending.

[351] Notwithstanding the finding of Jennes and Persyn (2015) that Belgian federal ministers appear –marginally- able to favour inhabitants of their electoral districts with more net formula-based fiscal transfers, including with more social allowances and/or with less social security contributions.

[352] Also the public sector wage bill could to an important extent be considered a case of permanent legislation. However, as said above, we were not able to calculate public expenditures net of wage expenditures over our sample period.

- When we exclude one by one each of the 9 governments from our sample, the significance of the coefficient on ministers in column (1) decreases, except that the p value of the coefficient on ministers decreases until below the 0.1 threshold when we exclude the Brussels Regional government (which does not show any variation in the number of ministers over our sample period). In contrast, the coefficient on ministers in column (2) remains significant when we exclude one by one each of the 9 governments from our sample. The exceptions are that the p value of the coefficient on ministers increases until above the 0.1 threshold when we exclude the Flemish government or the Walloon Regional government.
- What makes the p value in column (1) also improve slightly is the omission from our sample of by far the 4 smallest supra-local governments, i.e. German speaking Community, COCOF, VGC and GGC (see table 2), although this omission reduces our sample to 126 observations only. This omission however makes the p value of our coefficient of interest in column (2) increase above the 0.1 threshold.
- Limiting our sample to post-1995 years –as from then on there was no "double mandate" anymore, and as from then on each supra-local government of Belgium was directly elected- weakens the causal relationship suggested by columns (1) and (2): the coefficient on ministers reduces to 28 euros per capita, with a p value of 0.27, resp. to 90 euros per capita, with a p value of 0.17[353]. This weakening makes us suspect that it could have been the signature of the Maastricht treaty in 1992 on the criteria for accession to the Eurozone that may have caused the Federal government –as well as the simultaneous Regional and Community governments that were "symmetrical" in the partisan sense- to get a grip again on their expenditures and/or their budget balance, rather than the considerable reduction in Federal and Flemish government ministers after the 1991 elections (as shown by graph 4). Indeed, the Maastricht treaty was signed in February 1992, while the post-1991 elections Federal government was sworn in only in March 1992[354]. So perhaps the reduction in ministers happened not so much because the 1988 *"state reform" had been implemented already*. Indeed, another "state reform", be it of much smaller size, was voted during the 1992-95 legislature, thanks to the then Federal government having found parliamentary support for it outside the governing coalition. Hence perhaps the reduction in ministers rather happened because the 1992-1995 government precisely *wished to get control of expenditures*.
- In contrast to most of the existing empirical literature, we also repeated the regressions in table 7 while expressing not only our political variables but also all our other variables in absolute terms (and of course adding population as an extra control variable)[355]. Regression analysis then leads to the coefficient on ministers in columns (1) and (2) of table 7 to be positive and highly significant. This result is moreover robust to excluding the Federal government from our sample[356]. Also in contrast to most of the literature[357], we repeated the above regressions while expressing *all* our variables in per capita terms, i.e. while *also* expressing our political variables in per capita terms. The results then are that the coefficients on ministers in columns (1) and (2) become less

[353] In contrast, limiting our sample to the post-1988 years –the period for which we found expenditure data for most of the 9 supra-local governments- does not weaken the causal relationship suggested by columns (1) and (2).
[354] However, in contrast, the reduction in ministers of the Flemish government already took place in January 1992.
[355] An exception is Brooks e.a. (2011), who also conduct this sensitivity analysis.
[356] As well as to excluding every single government in a row from our sample, and to excluding the 4 smallest governments by far from our sample.
[357] An exception is Schaltegger and Feld (2009 p. 41).

significant. A similar result emerges if we express all our variables in logarithms, except our political variable of interest and our dummy variables.

- We have also considered whole legislatures as our time dimension, rather than individual years, as almost all our political variables of interest only change (i.e. are only decided upon) –if at all- right after elections. Only considering legislatures reduces our number of observations to 53, and makes the significance of the coefficients on the ministers variable diminish considerably[358].
- As another alternative, we *also* considered as exogenous the extra number of ministers and "cabinet members" in legislatures that *succeeded* "state reform" legislatures in case these numbers did *not* decrease in the succeeding legislature. In effect, in a limited number of occasions[359], the extra number of ministers and "cabinet members" due to "state reform" governments did *not or not fully* disappear when the next –non-"state reform"- government(s) came into power. See also graph 3 in annex 2. This may have been due to "habit formation"/inertia. When using these slightly different IVs for conducting our regressions in table 7, regression results are very much the same.
- As a final alternative, we expressed our variables in First Differences (FD) rather than to include government Fixed Effects (FE). Although FE is generally considered more efficient than FD, and although FE avoids losing one year of observations due to taking FD, Wooldridge (2009 p. 487) recommends FD over FE when the number of cross-sections is not very large (in our case only 9 governments), and when the number of time-units is large (in our case 23 years or more for most governments). Remarkably, when using FD instead of FE in table 7, the coefficients of interest in column (1) and (2) both become very insignificant[360]. This may be due to the fact that the FD transformation reduces variation in our data more than the FE transformation.

8. Conclusion

In this paper we investigate if a more populous or fragmented executive or a more populous or fragmented legislative increases government spending and/or the budget deficit, an effect known as the Law of 1/n (Weingast e.a. 1981). We test this for the supra-local governments of Belgium, i.e. the Belgian Federal government and the Belgian Regional and Community governments, as these are in charge of the bulk of public spending in Belgium. Possibly because our dataset is limited (as well as the number of Belgian supra-local governments as such), and because our identification relies on variation *over time* in the composition of the executive and the legislative of the various supra-local governments of Belgium, we do not find a significantly positive association between the sizes of the executive and the legislative on the

[358] Somewhat surprisingly, the entire surveyed empirical literature on the Law of 1/n makes only use of yearly data, with the exception of MacDonald (2008).

[359] With respect to ministers as well as "cabinet members": the Walloon Regional and the French Community government after the 1999-2004 "state reform" legislature. With respect to "cabinet members": the Federal government after the 1999-2003 "state reform" legislature.

[360] The FD transformation also allows us to include the lagged dependent variable (LDV) as a regressor, as expressing this –endogenous- variable in First Differences allows us to instrument it with its 2nd and 3rd lags in levels. However, when including the LDV in columns (4) and (5) of table 6 *without* instrumenting it, the ministers variable remains significant.

one hand and public expenditures on the other hand, nor between the sizes of the executive and the legislative on the one hand and the budget deficit on the other hand.

The exception relates to the size of the *executive*: we find a positive association between the number of ministers and public expenditures / the budget deficit. But we can only interpret the association between the number of ministers and the *budget deficit* in a causal way, as only our IV regression of the budget deficit on the number of ministers produces a coefficient that is statistically significant.

Our identification is based on an IV regression approach, instrumenting the number of ministers with their resp. extra number in governments *in charge of a "state reform"*. A "state reform" is a Belgian decentralisation round for which the support of a 2/3 majority in the Federal parliament is constitutionally required. This is a majority for which Belgian politicians mostly find it convenient to lay the foundations by inviting many parties to the governing coalition, in turn increasing the number of ministers and junior ministers.

We also find that a change in the size or fragmentation of the *legislative* has no effect on public spending, nor on the budget deficit. This is not very surprising as changes over time in the size of the legislative are less frequent than changes over time in the size of the executive, as in Belgium the executive is believed to dominate the legislative in terms of "fiscal power", and as Belgian MPs are elected under a proportional system, which seems to reduce the incentive to overspend on their electoral district compared to a system of single-MP districts.

Our investigation is most closely related to the one by Baskaran (2013) for German states. Not only do both studies focus on (1) supra-local governments only (2) within one and the same country. But also methodologically the studies are related, as they make an attempt at remedying the possible endogeneity of political variables. They therefore only rely on within variation for identification and moreover deploy an IV estimation approach. Also in terms of findings, both our and Baskaran's study find little evidence of a Law of 1/n holding for our sample. A difference between the 2 studies is unsurprisingly that Baskaran is able to make use of much more observations (16 states * 35 years).

Our overall conclusion should hence be that on the basis of our dataset –including rather few "cross-sections" as well as rather few changes in "n" over time- we find few indications that an increase in the number of politicians increases overall public expenditures (of course apart from increasing the direct expenditures on politicians) or that such an increase would increase the budget deficit. Reasons for this result could be our limited dataset, which is i.a. lacking time series on the number of and on the expenditures on private ministerial advisors in a political system of which it is believed that the executive strongly dominates the legislative, as well as the limited variation in the remaining political variables of interest. A possible *contents-wise* explanation of the overall lack of an effect goes back to Buchanan and Tullock (1962, quoted in MacDonald 2008). They argue that decision making costs increase with group size. Logrolling may become more difficult once a "group" has crossed a certain threshold with respect to its size. Apart from having increased the sheer number of politicians, the Belgian process of political decentralisation and fragmentation may have made the number of de facto "veto players" within the

Belgian political system so large that in a number of important respects the vote trading so typical in "veto" environments for legislation to pass may have become impossible[361].

References

Ade, F. and R. Freier, "When can we trust population thresholds in regression discontinuity designs?", Berlin, DIW Working Paper, 2011.

Aidt, T. and J. Shvets, "Distributive politics and electoral incentives: evidence from seven US state legislatures", American economic journal: Economic policy 4 (2012): 1-29.

Alesina, A. and R. Perotti, "The political economy of budget deficits". NBER working paper, 1994.

Alesina, A. and G. Tabellini, "Positive and normative theories of public debt and inflation in historical perspective", European Economic Review 39 (1992): 337-344.

Algoed, K., "De onderfinanciering van Brussel: een mythe?". Vives beleidspaper, 2009.

Alt, J., and R. Lowry, "Divided government, fiscal institutions, and budget deficits. Evidence from the states", American Political Science Review 88 (1994): 811-828.

Ashworth, J., B. Geys, and B. Heyndels, "Government weakness and local public debt development in Flemish municipalities", International tax and public finance 12 (2005): 395-422.

Banzhaf, J., "One man, 3.12… votes: a mathematical analysis of the electoral college", Villanova Law Review 13 (1968): 304-346.

Baqir, R., "Districting and government overspending", Journal of Political Economy 110 (2002): 1318-1354.

Baskaran, T., "Soft budget constraints and strategic interactions in subnational borrowing: evidence from the German states", Journal of Urban Economics 71 (2012): 114-127.

Baskaran, T., "Coalition governments, cabinet size, and the common pool problem: evidence from the German states", European Journal of Political Economy 32 (2013) : 356-376.

[361] The Belgian process of political decentralisation took to an important extent place over our sample period. However, as a matter of fact, "veto rights" were already established in Belgium by means of the 1st Belgian "state reform", that already took place in 1970:

- the legislative was divided into a "Dutch-speaking group" and a "French-speaking group", and any 2/3 of members of parliament of any group are since then entitled to block any law from being approved;
- the executive –which in Belgium decides by consensus- since then needs to consist of an equal number of ministers belonging to either parliamentary group

Moreover, during the 1970s every major Belgian political party split into a Flemish and a Francophone Belgian party, effectively doubling the number of parties needed to form a Federal government. As this government decides by consensus, also the number of "veto players" within this government doubled.

Bordignon, M., and G. Turati, “Bailing out expectations and public health expenditure”, Journal of Health Economics 28 (2009): 305-321.

Brooks, L., J. Phillips, and M. Sinitsyn, “The cabals of a few or the confusion of a multitude: the institutional trade-off between representation and governance”, American economic journal: Economic policy 3 (2011): 1-24.

Buchanan, J. and G. Tullock, “The calculus of consent”. Ann Arbor, University of Michigan Press, 1962.

de Coorebyter, V., « Les résultats des élections communales du 8 octobre 2006 à Bruxelles », Courrier hebdomadaire du CRISP 1980 (2007): 7-50.

de Haan, J., and E. Sturm, “Political and economic determinants of OECD budget deficits and government expenditures. A reinvestigation”, European Journal of Political Economy 13 (1997): 739-750.

Dewachter, W., “De mythe van de parlementaire democratie: een Belgische analyse”. Leuven, Acco, 2001.

Egger, P. and M. Köthenbürger, “Government Spending and Legislative Organization: Quasi-Experimental Evidence from Germany”, American Economic Journal: Applied Economics, 2 (2010): 200–212.

Eyraud, L. and L. Lusinyan, “Decentralising spending more than revenue: Does it hurt fiscal performance?” IMF Working Paper, 2011.

Freier, R. and C. Odendahl, “Do absolute majorities spend less? Evidence from Germany”. Berlin, DIW Working Paper, 2012.

Gagliarducci, S. and T. Nannicini, “Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection”, Journal of the European Economic Association 11 (2013): 369–398.

HRF, “Recente budgettaire evoluties en beoordeling ervan ten opzichte van het stabiliteitsprogramma”, 2013.

Huber, G., M. Kocher and M. Sutter, ”Government strength, power dispersion in governments and budget deficits in OECD-countries. A voting power approach”, Public Choice, 116 (2003): 333-350.

Inman, R. and M. Fitts, “Political institutions and fiscal policy: Evidence from the US historical record”, Journal of Law, Economics, and Organization 6 (1990): 79-132.

Inman, R. and D. Rubinfeld, “Rethinking federalism”, Journal of Economic Perspectives 11 (1997): 43-64.

Inman, R., “The flypaper effect”. NBER working paper, 2008.

Jennes, G., “Allocation of grants to Brussels municipalities: the effect of partisan alignment”. Vives briefing, 2014a.

Jennes, G., “Has Belgian fiscal decentralisation reduced the size of government and the budget deficit ?” Vives discussion paper, 2014b.

Jennes, G. and D. Persyn, “The effect of political representation on the geographic income distribution in Belgium: evidence using Belgian data”, European Journal of Political Economy 37 (2015): 178-194.

Lago-Peñas, S., A. Sacchi, and P. Simon-Cosano, “Who honor the rules of federalism? Party system nationalisation and fiscal performance”. Universida de Vigo, GEN working paper, 2014.

MacDonald, L., “The impact of government structure on local public expenditures”, Public Choice 136 (2008): 457-473.

Marcel, M., “Budgeting for fiscal space and government performance beyond the great recession”. Paris, OECD, 2012.

Niskanen, W., “Bureaucracy and Representative Government”. Chicago & New York, Aldine-Atherton Inc., 1971.

Perotti, R., and Y. Kontopoulos, “Fragmented fiscal policy”, Journal of Public Economics 86 (2002): 191-222.

Persson, T. and G. Tabellini, “Political economics”. MIT Press, 2000.

Pettersson-Lidbom, P., “Testing political agency models”. University of Stockholm Working Paper, 2006.

Pettersson-Lidbom, P., “Dynamic commitment and the soft budget constraint: an empirical test”, American Economic Journal: Economic Policy 2 (2010): 154-179.

Pettersson-Lidbom, P., “Distributive politics and electoral incentives: evidence from seven US state legislatures: a comment”. Stockholm University, 2012a.

Pettersson-Lidbom, P., “Does the size of the legislature affect the size of government? Evidence from 2 natural experiments”, Journal of Public Economics 92 (2012b): 269-278.

Primo, D. and J. Snyder, “Distributive politics and the Law of 1/n”, Journal of Politics 70 (2008): 477-486.

Rodden, J., “Hamilton’s Paradox: The Promise and Peril of Fiscal Federalism”. MIT, 2005.

Roubini, N. and J. Sachs, “Political and economic determinants of budget deficits in the industrial democracies”, European Economic Review 33 (1989): 903-938.

Schaltegger, C., and L. Feld, “Do large cabinets favor large governments? Evidence on the fiscal commons problem for Swiss cantons”, Journal of Public Economics 93 (2009): 35-47.

Smulders J., J. Faniel , and B. Maddens, “Verkiezingen, partijen en overheidsfinanciering: de financiële gevolgen van de verkiezingen van 25 mei 2014”, Tijdschrift voor Bestuurswetenschappen en Publiekrecht (2014): 428-437.

Sorribas-Navarro, P., “Bailouts in a federal system: evidence from Spain”, European Journal of Political Economy, 27 (2011): 154-170.

Spolaore, E., “Policy making systems and economic efficiency: coalition governments versus majority governments”. ECARE, 1993.

Timmermans, A., “Cabinet ministers and coalition formation in Belgium: the impact of coalition constraints”, in: Laver, Michael and Kenneth Shepsle (eds.), “Cabinet ministers and parliamentary government”. Cambridge University Press, 1994.

Velasco, A., “A model of endogenous fiscal deficits and delayed fiscal reforms”. NBER working paper, 1997.

Volkerink, Bjorn and Jakob de Haan, “Fragmented government effects on fiscal policy. New evidence”, Public Choice 109 (2001): 221-242.

Weingast, B., K. Shepsle, and C. Johnsen, “The political economy of benefits and costs: A neoclassical approach to distributive politics”, Journal of Political Economy 89 (1981): 642-664.

Wooldridge, J., “Introductory econometrics”. South-western, 2009.

Annex 1: Correlation between the 9 political variables of interest after demeaning

The correlations below are calculated after subtracting both the within and between averages of all our political variables, as the variation contained in these twice demeaned variables comes close to the variation in political variables we use for identification while including Fixed Effects as well as year dummies in our regression specifications in tables 6 and 7 above.

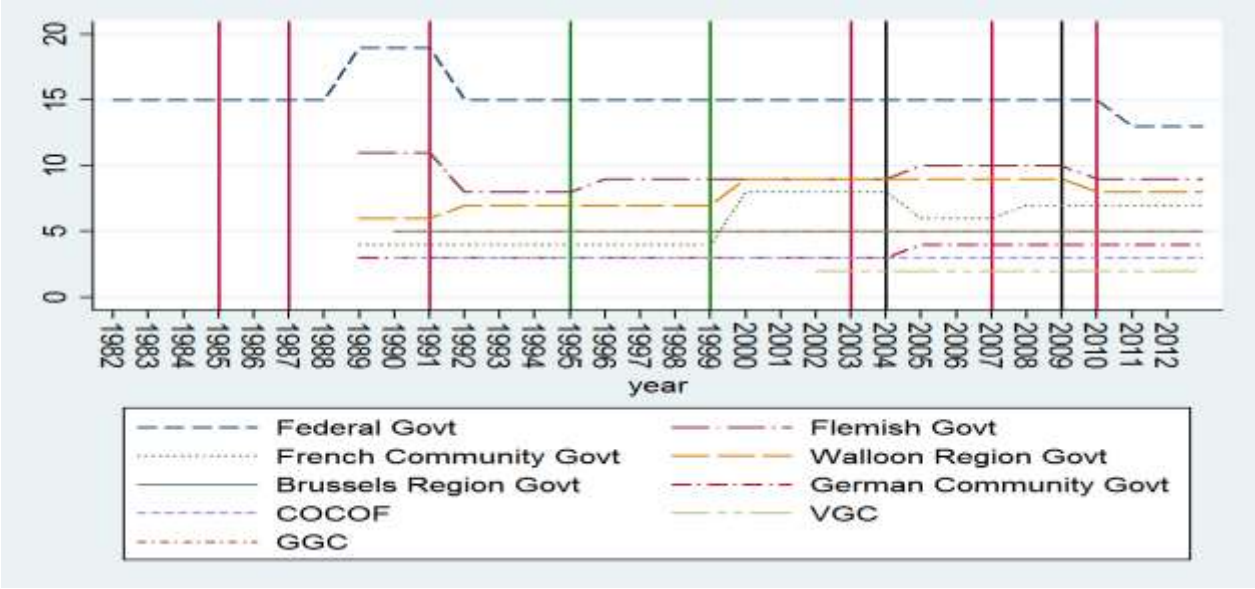
	Ministers	"Cabinet members"	Parties in the coalition	Executive's Herfindahl	Govt friendly MPs	Ministers' "second jobs"	Electoral districts	MPs	Parties in parliament	Legislative's Herfindahl
Ministers	1									
"Cabinet members"	0.9780	1								
Parties in the coalition	-0.1074	-0.0738	1							
Executive's Herfindahl	0.0234	0.0261	-0.7407	1						
Government friendly MPs	0.3101	0.2950	0.4998	-0.4503	1					
Ministers' "second jobs"	0.1579	0.1382	-0.0554	-0.0281	0.0536	1				
Electoral districts	0.3130	0.3326	0.4014	-0.2374	0.5568	0.0019	1			
MPs	0.1651	0.1845	0.4893	-0.3197	0.5820	-0.0028	0.9674	1		
Parties in parliament	-0.0123	0.0446	-0.3631	0.3659	-0.4944	0.0482	0.1180	0.0581	1	
Legislative's Herfindahl	-0.0050	-0.0430	-0.0921	0.2957	0.1976	-0.0142	-0.1524	-0.1980	-0.5908	1

Annex 2: The evolution of the number of ministers shown graphically

Graph 3 shows between and within variation for our number of ministers variable. It does not show its evolution during years for which we have no expenditure data available, as the observations concerned are obviously dropped from our regression analysis. Note that by definition values taken by the number of ministers variable are the same for the Brussels Regional government and the GGC, as the GGC is governed by exactly the same politicians as the Brussels Regional government, as explained above.

The Federal government is governed by far by the highest number of ministers. Graph 3 shows that – while the number of ministers is rather stable over time for most governments- 5 governments out of 9 experience some within variation over our sample period. However, unlike expenditures per capita as shown by graph 1, the number of ministers per government does not show a clear increase over time (as was already suggested by table 4a above). The hump in Federal ministers characterising the 1988-1991 legislature was related to the major "state reform" that took place during this legislature. Although the number of ministers in charge of "state reform" during this legislature did not differ from the previous nor from the subsequent legislature, the large number of ministers is to be understood as a consequence of the very large government coalition that was –painstakingly- established after the 1987 elections with a view to being able to rely on the 2/3 majority in the federal parliament that is constitutionally needed for a "state reform" in Belgium. The 1988-91 Federal government coalition was supported by 150 out of 212 members of the House of Representatives.

Graph 3: Ministers per government (1982-2012)



Overall conclusion

In most of what preceded, we have analysed the extent to which the geographic distribution of fiscal transfers in Belgium matches the geographic distribution of political power and of government popularity. Only in chapter 5 we analysed the extent to which *overall* public spending matches *overall* political representation. 4 of our 5 chapters have in common that they are an attempt to deal with a number of constraints imposed on panel data analysis that are typical to several studies on *supra-local governments* in general, and typical to several studies on a rather small country like Belgium in particular. With respect to Belgium being a rather small country, a consequence of it is that Belgium has rather few geographic regions (and rather few supra-local governments) and hence "few cross-sections" to base a panel data analysis on. Hence in most of the chapters a challenge has been to collect sufficient variation at the "cross-sectional level" to base a panel data analysis on. In the 1st chapter we have focused on the electoral district level for "cross-sectional" variation, in the 3rd chapter on the administrative district level, in the 4th chapter on the municipal level, and in the 5th chapter on the supra-local government level.

The 4 panel data analyses have also in common that all of them are an attempt to deal with a number of constraints that are typical to the empirical analysis of the effect of *political economy factors* in general. With respect to this 2nd type of constraint, the major challenge is that those political economy factors are generally speaking endogenous. Effectively, the fact that a particular electoral district is assigned a Federal government minister (chapter 1), the fact that in a particular municipality a political party is in power (chapter 4), the fact that a particular Belgian supra-local government is composed of more ministers than average (chapter 5), and the fact that a particular administrative district receives a particular amount of fiscal transfers (chapter 3) may all be endogenous. While a Regression Discontinuity Design (RDD) would have been the preferable approach to dealing with such endogeneity, the Belgian setting combined with our particular research questions each time prevented us from identifying a "useful" discontinuity, either because we simply found such a discontinuity lacking or because we observed too few data points situated in the neighbourhood of the discontinuity in question.

Therefore in each of the abovementioned chapters we resorted to a more traditional Instrumental Variables (IV) estimation. This empirical strategy forced us in each chapter to come up with credible instruments, which in our view also happened to be original instruments against the background of the existing literature: ministers "lost" by a district following a conflict over policy or a political "scandal" and ministers "obtained" by a district following an electoral district reform (chapter 1), Brussels municipal coalitions that are "anti-coalitions" formed against the traditionally dominant party, or that comprise a party that had never before been in power (chapter 4), governing coalitions that were extended because of the Belgian constitutional requirement to assemble a 2/3 majority in the federal parliament to be able to implement a "state reform" (chapter 5), and an increase in fiscal transfers that was the result of the downsizing of a company or of the 2008 worldwide economic crisis (chapter 3). Using (types of) instruments that had not been used before in the literature may be considered as a way of usefully extending the existing political economics literature concerned also from a methodological point of view[362].

[362] As is recommended by Besley and Case (2003 p. 34).

A final constraint we had to cope with is that in Belgium geographically disaggregated data on taxing and spending that are publicly available are rare[363]. For our 1st and 3rd chapter we have gratefully exploited the geographic disaggregation of important federal taxing and spending categories down to the district level that are made publicly available by the annually published "Regional accounts" of the NBB. In contrast, for the 4th chapter we had to exploit non-published intergovernmental grants data per municipality that had been kindly made available by a source outside government. For the 5th chapter we had to manually copy the aggregate expenditure data per Belgian supra-local government from the federal Ministry of Finance's "Conjunctuurnota", as the Federal government appeared unable to provide us with these data electronically, and as our enquiry addressed at every single Belgian supra-local government had not provided us with aggregate expenditure data that go back to an earlier year than 1989.

The fact that until now, the effect of political economy factors on supra-local taxing and spending in Belgium had never been investigated may seem surprising, given the salience of political economy factors in Belgium. Indeed, its size of government, its size of public debt, its number of politicians, and its political, cultural and economic heterogeneity and even divergence may be considered large from an international perspective. However, the methodological challenges that we have encountered make this scarcity less surprising. What may remain surprising is that in most of our chapters we find either evidence that is only strong in a limited number of specifications, or strong evidence of a rather limited economic significance with respect to any causal link between the geographic distribution of political power and the geographic distribution of fiscal transfers in Belgium.

While we find strong evidence that the average federal minister steers net formula-based fiscal transfers to citizens of his/her electoral district, we find not as strong evidence that public spending and the budget deficit of a Belgian supra-local government increase with its number of ministers. We also find that only *some* political parties –as opposed to *other* political parties- in power at a Belgian supra-local level tactically favour those municipalities where they are also in power with extra discretionary transfers. Finally we only find weak evidence that an increase in net fiscal transfers to citizens would make Flemish citizens vote less in favour of the incumbent Federal government than Francophone Belgian citizens, given that the burden of any debt increase following such a net fiscal transfers increase would be disproportionately borne by Flemish citizens.

However, one could also take the viewpoint that the evidence found is surprisingly strong, given our limited number of observations, given that we only identify on –in a number of cases limited- within variation (while excluding between variation as a basis for identification), and given that we consistently use IV estimation to isolate a causal relationship between our respective independent variables of interest and our dependent variables. Therefore also, it is a matter of further research if the limited evidence we find is due to lack of data, the limited number of observations available, our exclusive identification on within variation in our variables of interest, a yet insufficiently innovative empirical approach, or an effectively rather weak impact of the political economy factors concerned.

Reference

Besley, T. and A. Case, "Political institutions and policy choices", Journal of Economic Literature 41 (2003): 7-73.

[363] And that in case they are available, they are mostly only available over a rather short time period.

The list of doctoral dissertations from the Faculty of Business and Economics is available at http://www.kuleuven.be/doctoraatsverdediging/archief.htm